

Diachronic Evidence for Synchronic Analyses  
in Phonology\*

Donald G. Churma

1. Introduction. There is currently little agreement among phonologists as to the form which phonological rules and phonological representations in 'correct' grammars of natural languages should take. The term 'correct' is presumably understood by most investigators as being at least roughly equivalent to what Chomsky (1964, 1965) calls "descriptively adequate." This latter concept ("descriptive adequacy") is, according to Chomsky (1964:105), to be equated with what Sapir (1933) terms "psychological reality" (cf. also Chomsky 1976). That is, modern phonologists, like (among others) Sapir and Chomsky, are concerned with determining the form in which speakers of a language store in long term memory the pronunciations of the lexical items of their language, and the form of the rules (if any) which convert the stored pronunciation into the actual pronunciation. To put it in slightly more technical terms, phonologists today are concerned with determining the nature of lexical representations (also referred to as underlying representations or underlying forms) and of phonological rules in descriptively adequate grammars of natural languages.

Such a concern is, perhaps contrary to popular belief, not peculiar to generative phonology and those theories which have been influenced by and/or have reacted to generative phonology (e.g., the "natural phonology" of Stampe 1973, Donegan and Stampe 1979, and the "Natural Generative Phonology" of Hooper 1976 and the references cited there). Rather, as pointed out by Chomsky (1978:304-5), most American structuralists, for example, implicitly accepted such a concern as legitimate in linguistic research, despite the probability that the great majority of them would have rejected a proposal of this nature if it was explicitly put to them. This must be so because the sequence of papers which appeared shows that a set of procedures for phonemic analysis, say, would be proposed, another investigator would point out that using these procedures would lead to an absurd result in certain cases and perhaps propose a revision of the original principles, etc. Unless these investigators were implicitly accepting a point of view very much like that which concerns itself with descriptive adequacy, there would appear to be no basis for claiming that the results in question were in fact absurd. Descriptive adequacy has thus been an implicit concern in most of those phonological theories in which a concern of this nature has not been explicit, as it has been in the case of generative phonology and the other current theories mentioned above.

Moreover, it seems to me, it is absolutely crucial that this be the main concern of phonologists, unless one is willing to claim that the only concern of phonology is the specification of the actual pronunciation of utterances. The achievement of even the latter end is, of course, no mean accomplishment, but by itself it is of little or no relevance to ultimately more important and interesting questions having to



do with the nature of the human mind. That is, the ultimate concern of generative phonologists, in my view (and, at least implicitly, that of most other phonologists), is the light which the study of the phonological part of human language can shed on the nature of the mind. Such light simply cannot be shed unless we go beyond the mere specification of the pronunciation of utterances to concern ourselves with the way in which these pronunciations come about when speakers of natural languages produce them.

Despite the importance of the issues addressed by modern phonology, there is, as noted above, little agreement concerning the resolution of these issues. Probably the most important reason for this lack of agreement is that phonologists until quite recently (with some notable exceptions such as Sapir), have taken as their data almost exclusively the facts of pronunciation in various natural languages. For, while no one can deny that such facts must be considered when attempting to answer the questions discussed above, neither, it seems to me, can it be denied that there are other data which, if they are available, cannot be neglected if we are to find satisfactory answers to our important questions. It has been well known at least since Chao (1934) (and undoubtedly well before then) that pronunciation data alone cannot in general supply a unique answer to phonological questions, even given rather specific assumptions about the form which such answers may take (e.g., the assumptions of classical phonemics).

It is this lack of uniqueness for solutions of phonological problems, of course, which has led to the current widespread disagreement among phonologists as to the correct answers to their questions. Recently (Sapir can again be considered 'recent' in this respect), there have been several attempts to resolve these difficulties by appealing to data other than the pronunciations of utterances themselves. In my opinion, it is evidence of this type (which has been termed "external evidence" by, e.g., Botha 1973, and "substantive evidence" by such researchers as Skousen 1975) which is required to settle the disputes which have arisen concerning whether or not proposed grammars (or parts of them) are descriptively adequate.<sup>1</sup>

Moreover, it appears to me, it is issues of this kind (i.e., at the level of descriptive adequacy) which can most profitably be pursued, given the current state of our understanding of the nature of language. It makes little sense to concern ourselves with the admittedly ultimately more philosophically profound issue (cf. the above discussion of "the nature of the human mind") of "explanatory adequacy" (cf., for example, Chomsky 1964:63) of theories of language which provide "a general basis for selecting a grammar which achieves" the still unclear level of descriptive adequacy. The actual practice of contemporary phonologists bears out this assessment: most current theoretical research is concerned with the descriptive adequacy of various proposed grammars, and not with constructing explanatorily adequate theories. Such a concern is entirely appropriate in my view: determination of the nature of descriptively adequate grammars seems clearly to be logically prior to work on explanatorily adequate theories.

Despite the apparently clear desirability of using "external" or "substantive" evidence (hereafter, simply 'external evidence') to decide phonological issues, there is little consensus among phonologists concerning the success of arguments employing such evidence. One major reason



(and perhaps the most important) is that the premises of these arguments have been left, in many cases, implicit. When the premises of an argument are not made explicit, it is difficult to evaluate it, since the truth of the premises is a crucial concern.<sup>2</sup> It is therefore of considerable importance that these premises be made explicit, insofar as possible, so that they may be evaluated with respect to their truth or falsity (or, probably more appropriately, the likelihood of these premises). Once the premises of an argument have been made explicit, of course, it can be determined whether or not a logically valid form of inference is being employed, as well.

In this paper, I will examine one kind of such evidence, data from language change,<sup>3</sup> and attempt to evaluate its relevance for determining the descriptive adequacy (i.e., psychological reality) of synchronic analyses in phonology. This kind of external evidence is particularly important, it seems to me, since, first of all, it has been used probably more frequently than any other kind, but also because, as noted by Sommerstein (1977), it is perhaps the only kind of external evidence which is generally accepted as being relevant to synchronic analysis. The approach taken here is a methodological examination of four arguments which have been put forth in favor of certain phonological analyses, together with replies to them and, in one case, a methodological critique. In so doing, I will attempt to make explicit the premises on which these arguments are based, and to evaluate these premises as potential 'universals' of language change which could serve as a firm basis for using diachronic data in assessing synchronic analyses.<sup>4</sup> Such an investigation, I feel, is long overdue, given the importance of the issues and the type of evidence involved, as well as the essential lack of any comparable work.<sup>5</sup> Before proceeding with the investigation, however, I will first give a brief sketch (section 2) of my background assumptions about the nature of phonological systems, and (section 3) of a framework for discussing and evaluating scientific arguments.

2. Background assumptions. In a work of a methodological character, it is well to make as few assumptions as possible, since the entire undertaking could be compromised if one of the assumptions accepted should prove false. However, it is extremely difficult, if not impossible, to do work (even when it is methodological work) in a theoretical vacuum, and I will take this opportunity to specify what will be assumed here regarding the nature of the phonological component of grammars of natural languages.

The assumptions made are given in (1) below. The last assumption (1c) actually follows from (1a) and (1b), but I include it here for the sake of clarity.

- (1) a. the physical speech signal is, at least partially, segmentable into discrete sequential units<sup>6</sup> (i.e., there is a level which corresponds roughly to the "systematic phonetic" level of Chomsky and Halle 1968)
- b. there may be a level more 'abstract' than the level just specified in that representations at the former level need not correspond one-to-one to those at the latter (the former level will be referred to here as the level of 'lexical representation,' a level which



may or may not be the same--perhaps depending on the lexical item--as the systematic phonetic level)

c. due to the potential discrepancy between the level in (a) and that in (b), there may exist 'phonological rules' which convert representations at the latter level into representations at the former level.

Many of the arguments in the literature which make use of external evidence are concerned with ascertaining the nature of the level of lexical representation, and this work will correspondingly be primarily concerned with this issue.<sup>7</sup> This issue is quite an important one, since resolving it often results automatically in the resolution of other issues, such as whether or not we are dealing with a case of "rule inversion" (cf. Venne-mann 1972 and section 4.3 below), but it will not be my exclusive concern.

3. On the nature of scientific argumentation. In Churma (1979, Ch. II), I argued that many scientific arguments can be understood as instances of what has come to be known as Bayesian inference (cf., for Example, Salmon 1967). The logical basis of this form of inference is in probability theory, and it can easily be shown (see Churma 1979, Appendix) that a formula corresponding to this form of inference follows straightforwardly from the axioms of the probability calculus. It can, of course, be quantified, but for present purposes, it will suffice to characterize Bayesian inference as in (2).

- (2) Unless hypothesis H is true, it would be unlikely that piece of evidence E would occur.  
E does occur.  
Therefore, it is likely that H is true.

Of crucial importance here is how the "unlikely" nature of E is to be determined. I have argued (in some detail, since this particular view does not seem to have been seriously considered in the logico-philosophical literature) in Churma (1979, Ch. II) that such determination is ultimately subjective in nature. Among other things, this view permits an explanation of how it is possible for rational investigators to disagree about the force of a given argument: they disagree because of incompatible beliefs (or degrees of belief) about the truth of the major premise in (2). The following section of this paper can also be taken as an argument in favor of this view.

Another straightforward consequence of the axioms of the probability calculus (cf. Churma 1979, Ch. II and Appendix) is what I have called "'almost deductive' inference". This kind of inference resembles standard deductive inference (in particular, modus ponens and modus tollens), but here the truth of the premises need not be known more than probabilistically. Thus, in the case of 'almost modus ponens', we have the situation illustrated in (3).

- (3) If A, then B is likely.  
A is likely.  
Therefore, B is (somewhat) likely.<sup>8</sup>



'Almost modus tollens' takes an analogous form. Let us turn now to an examination of the use of data from diachrony in the justification of synchronic analyses.

4. Appeals to diachronic evidence. In this section I will examine four sets of arguments which make use of evidence from historical change, and replies which have been made to each of them. After reconstructing in more explicit fashion the arguments and counterarguments, I will give some discussion of the validity of the argument forms used and (more often) of the truth/falsity or likelihood of the premises on which these arguments are based. The arguments considered are found in Kiparsky (1968), Hooper (1976), Schuh (1972) and Skousen (1972, 1975); the rejoinders are from, respectively, Stampe (1973), Harris (1978), Leben (1974) and Kiparsky (1973a, b). A "remark" on Leben's reply by Schuh (1974) is also given some attention, as is Botha's (1973) methodological critique of Kiparsky's argument. After each presentation of the relevant arguments, I will give some discussion of the cogency of the arguments in question. In the final section, I will discuss the important issue of possibly taking seriously some of these premises (or generalizations of them) as universal principles of historical change which could serve as a legitimate basis for other arguments which attempt to justify synchronic analyses on the basis of data from diachrony.

#### 4.1. Kiparsky-Stampe-Botha.

4.1.1. Kiparsky on the brace notation. I will consider first of all the argument presented in Kiparsky (1968) (and reiterated in Chomsky and Halle 1968) in favor of the psychological reality of the brace notation. This argument is noteworthy in that it has received extensive, and essentially negative, discussion by Botha (1973). It is also a particularly transparent application of the Bayesian schema (1). Furthermore, Stampe's (1973) reply (discussed in the next subsection) explicitly acknowledges "the beauty of Kiparsky's argument" (Stampe 1973:48), although Stampe argues against Kiparsky's conclusion. It thus appears worthwhile to consider this argument in some detail; I reproduce below verbatim in (4) Kiparsky's argument (the numbering in the quotation is Kiparsky's).

- (4) In English, underlying long vowels, which are otherwise realized as diphthongs, are shortened in two main phonological environments: before two or more consonants (for example, keep:kept) and in the third syllable from the end of the word (for example, vain:vanity, severe:severity). The rules which bring these shortenings about are the following:

- 5'.  $V \rightarrow [-\text{long}] / \_\_\text{CC}$   
5''.  $V \rightarrow [-\text{long}] / \_\_\text{C...V...V}$

The theory of generative grammar requires that 5' and 5'' be collapsed into a single rule as follows:

5.  $V \rightarrow [-\text{long}] / \_\_\text{C}^{\text{C}}\{\text{...V...V}$

It asserts that of the two descriptively equivalent grammars, one of which contains the two rules (5' and 5'') as separate processes, and the other as a single



process combined into 5 by factoring out their common part and enclosing the remainder in braces, it is the latter which is the psychologically correct one.

Rule 5 arose in Early Middle English as a generalization of a much more restricted process of shortening. In Old English, vowels were shortened before three or more consonants (for example, gōdspell > godspell, bræmbias > bræmbias) and in the third syllable from the end provided they were followed by two consonants (for example, blēdsian > bledsian). The corresponding rules were:

- 6'. V → [-long]/\_\_\_ CCC  
6''. V → [-long]/\_\_\_ CC...V...V

Again, these rules must be collapsed as before:

6. V → [-long]/\_\_\_ CC{<sup>C</sup>...V...V

On comparing the Old English rule in 6 and the Early Middle English (and indeed Modern English) rule in 5 we see that the only difference between them is that the later rule (5) has lost one of the required consonants in its environment. It represents a simpler, more general form of the Old English vowel-shortening process. It will apply in all cases where 6 applied but also in cases where 6 would not have applied. Evidently the change from 6 to 5 is an instance of simplification, which we have seen to be one of the basic mechanisms of linguistic change. But in a linguistic theory in which the brace notation plays no role, the relation between the Old English and Early Middle English shortening process is a different one. If the brace notation were not part of linguistic theory we would have two separate changes--namely, 6' > 5' and 6'' > 5''--on our hands and we would be faced with the very peculiar fact that two separate, unrelated rules have undergone an identical modification at the same point in the history of English.

The last sentence of this passage bears a strikingly close relationship to the major premise in the Bayesian schema (2). I rephrase this sentence slightly here to emphasize the parallel, as in (5) below.

- (5) Unless the brace notation were a part of linguistic theory, it would have been unlikely for the phonological system of Old English to have changed into that of Early Middle English in the way that it did.

It is clear that Kiparsky regards such a change as quite unlikely from his characterization of it as a "very peculiar" one. When we add as a minor premise a statement that such a change did in fact occur, of course, it follows from (1) that the brace notation is (strongly) supported (the more unlikely the change in question is considered, the stronger the support offered by the fact that it did occur), as long as we agree with these premises.<sup>9</sup>



4.1.2. Stampe on Kiparsky's argument. Stampe (1973:48) begins his critique of Kiparsky's argument by, as noted above, "granting the beauty of this argument..." I take this as implying that Stampe, at least, feels that the argument form employed by Kiparsky is a legitimate one (as well as a "beautiful" one). This, despite the fact that Stampe does not accept Kiparsky's conclusion, is an indication of the strong intuitive appeal of the Bayesian argument schema.

What apparently motivated Stampe to question the soundness of Kiparsky's argument (though, again, not the validity of the argument form) seems to be that it "seems odd that phonetically motivated changes of this sort, changes which at least in their inception were imposed on the language by its speaker, and not vice versa, should be subject to the sort of cognitive analysis implied by the brackets" (Stampe 1973:48). In terms of the Bayesian schema (1), even if the brace notation were psychologically real, changes of this type would be unlikely.<sup>10</sup>

Stampe goes on to present an alternative analysis of the facts of Old English and Early Middle English, one which would not make a change of the type that occurred appear unlikely, thus disputing Kiparsky's (implicit) claim about the unlikelyhood of this change without the psychological reality of braces. In so doing, however, he not only disputes the value of the probability that E is true unless H is true; he also calls into question whether or not E is actually true. Thus, instead of Kiparsky's rules, Stampe (pp. 48-9) has (6) and (7) for Old English and Early Middle English, respectively.

(6)  $V \rightarrow \text{-long}/\_\text{CC}$ . (where . is a syllable boundary)

(7)  $V \rightarrow \text{-long}/\_\text{C}$ .

That is, in Old English vowels were shortened in a syllable which was closed by two consonants, while at the later stage only one consonant was required for the shortening to take place. In favor of this formulation of the rules, Stampe (p. 49) adduces counterexamples to Kiparsky's analysis which are in accord with his own (OE hīehsta 'highest', EME rēspondent, where the st and sp are syllabified with the following syllable). Thus the evidence on which Kiparsky bases his argument is not only unlikely, but does not occur. That is, the psychologically real grammar of Old English contained neither the collapsed rule 5 nor the two separate rules 5' and 5'', and similarly for that of Early Middle English.

It is worth pointing out that the evidence in this case is not unambiguous observational data,<sup>11</sup> but linguistic analyses which may or may not be correct. It should not be surprising, then, if one investigator finds that he disagrees with another concerning whether or not the 'evidence' cited in an argument actually obtains. What needs to be done in a case like this is to find independent evidence for preferring one of the analyses over the other, as Stampe has done in this case by offering the counterexamples noted above. Something analogous is true, I would maintain, in the case of many major premises.

4.1.3. Botha on Kiparsky's argument. Botha (1973:94-111; 136-66) has also subjected Kiparsky's argument to some criticism, but on grounds quite different from those offered by Stampe. Using the framework developed in



Toulmin (1964), Botha "reconstructs" this argument (pp. 100-1) as in (8).

- (8) If the brace convention...is incorporated in the general-linguistic theory, then it follows that the linguistic changes  $C_1$  and  $C_2$  could have occurred as a single unitary change  $C$ .  
The linguistic changes  $C_1$  and  $C_2$  occurred as a single unitary linguistic change.  
∴ The brace convention...should be incorporated in the general-linguistic theory and should be assigned psychological reality.

Botha is unhappy with this argument because (p. 101) "there is a distinct qualitative difference in content between the evidential statement, which as the minor premise refers to anhistorical and diachronic state of affairs and the second half of the conclusion, in which a claim is made about a nonhistorical, nondiachronic mental state of affairs". But, as noted in section 4.1.1, the first and second halves of the conclusion appear to be considered logically equivalent by Kiparsky, and therefore the second half must not be qualitatively different unless the first one is.

There are further problems with Botha's reconstruction, and I would like to give a brief discussion of this issue, in the course of which it should become clear that the entire argument as reconstructed is incoherent unless Kiparsky's views on psychological reality as it relates to what is part of the "general-linguistic theory" are accepted, and, in fact, it seems that any argument from external evidence (in particular, historical change) implies acceptance of quite similar views.

It is somewhat difficult to interpret this reconstruction, since it seems paradoxical to me to speak of more than one linguistic change (here " $C_1$  and  $C_2$ ") having "occurred as a single unitary change." What Botha may mean by this is that we can use the brace notation to formally express as a "single unitary linguistic change" what is actually two separate ones. But if this is the case, then it is not clear that even the first conjunct of the conclusion is qualitatively relevant to the premises: why should linguistic theory incorporate a device that enables us to write as one change what is in reality two changes?

Let us suppose, then, that what Botha means here is something like 'what otherwise would appear to be separate changes can be expressed as the "single unitary linguistic change" that it in fact is,' since this seems to be the only other alternative for resolving the paradox. This appears to be what both Kiparsky and Stampe are arguing--that there is a single change involved, not two.

But if we accept this, there is a problem concerning the major premise: why should we expect the incorporation of a purely descriptive theoretical device into the "general-linguistic theory" to have any inductive consequences concerning possible linguistic changes? That is, if the brace convention is a purely descriptive device (i.e., if it does not have psychological reality), then the premises are not relevant at all to actual linguistic changes, but only to how we can describe them. Thus, in order to get a reasonable major premise, we must attribute psychological reality to the brace convention--we must be willing to claim that it is more than just a descriptive device. Since Botha's reconstruction is considerably changed by now, I give below in (9) a re-reconstruction on the basis of the preceding discussion.



- (9) If the brace convention is psychologically real, then it follows that what otherwise would appear to be separate linguistic changes are in fact a single unitary linguistic change.

What otherwise would appear to be separate linguistic changes are in fact a single unitary linguistic change.

∴ The brace convention is psychologically real.

This, I feel, is somewhat closer to what Kiparsky intended, and it can be seen that Kiparsky's conclusion is the result of a perfectly legitimate (inductive) argument. (Of course, Stampe's counterarguments would still hold, since under his analysis the single change would no longer otherwise appear to be separate ones.) But even given this version, there is no provision in the reconstruction for how strongly the evidence supports the conclusion (recall that the argument is a nondemonstrative one, and therefore does not establish the conclusion), and both Kiparsky and Stampe apparently feel (cf. the latter's granting of the "beauty" of Kiparsky's argument) that, if the premises are granted, the conclusion would be strongly supported. What is missing here is a reference to how likely it would be for the apparently different linguistic changes to in fact be one even if the brace notation did not have psychological reality, a reference which can be readily seen in the Bayesian schema (2), but which would seem to be difficult to incorporate into any reconstruction of the type that Botha gives. That is, Kiparsky's argument is reconstructible much more appropriately in terms of a Bayesian schema than in Botha's terms. Thus, the form of Kiparsky's argument is quite legitimate, although Stampe has given reason to doubt the premises involved.

#### 4.1.4. Discussion. What can be concluded from the above considerations?

It seems to me that Kiparsky is quite correct in his contention that a certain aspect of linguistic theory is supported insofar as it renders an otherwise unlikely phenomenon comprehensible. This is just an instantiation of Bayesian inference, and as such has the same formal characteristics as many other (non-linguistic) scientific inferences; the argument form is a valid one. In this particular case, unfortunately, it seems likely that at least one of the premises involved is not true, and so, of course, the conclusion is not supported by the premises. It is worth pointing out here that, although in this case the determination of the required likelihoods seems relatively straightforward, such determination will in many cases be pretty clearly subjective; indeed, even in this case, a convinced opponent of the 'reality of the syllable' would undoubtedly find the issue much less straightforward than I have implied it to be.

#### 4.2. Hooper-Harris.

4.2.1. Hooper on vocalic alternations in Spanish. Hooper is concerned with certain vowel alternations found in Spanish verb forms, and how these alternations should be treated in a synchronic description of Spanish. The alternations in question are: (i) mid vowel/diphthong (cf. t[e]ndémos/t[yé]nden 'we/they tend', c[o]cémos/c[wé]cen 'we/they cook'); (ii) high vowel/mid vowel (pedímos/píden 'we/they ask for'); and (iii) high vowel/mid vowel/diphthong (mintiéran 'that we lie' /mentímos/m[yé]nten 'we/they lie'). Following Hudson (1974, 1975), Hooper proposes to treat these alternations as "suppletive" in nature, so that the verb stem for tend,



for example, would be represented in the lexicon as  $t\{\overset{e}{ye}\}nd$ . A rule is then formulated which specifies the environments in which each of the alternants is found. The rule which Hooper eventually settles on is given in (10) (Hooper's 29 p. 159).

$$(10) \quad \left\{ \begin{array}{l} ye/we \\ e/o \\ <i/u> \end{array} \right\} \rightarrow \left\{ \begin{array}{l} ye/we / [+stress] \\ e/o </\_\_C_o i> \\ <i/u> \end{array} \right\}$$

What this means is that if a lexical entry contains a  $\{\overset{ye}{e}\}$ , then ye appears under stress, while e appears elsewhere (the o/we alternation is precisely analogous in this respect); if there is a  $\{\overset{e}{i}\}$  in the lexical entry, then e appears if the following vowel is i and i appears elsewhere; and if we have a  $\left\{ \begin{array}{l} ye \\ e \\ i \end{array} \right\}$ , ye is found under stress, e if unstressed and an i

follows, and i otherwise. Hooper's historical argument is an attempt to support this analysis, and thereby Hudson's "suppletive" theory of lexical representation on which it is based. Let us now examine this argument.

These alternations have undergone analogical leveling in some dialects of Spanish, so that only the diphthong appears where formerly there was a mid vowel/diphthong alternation, Hooper states. In addition, the high/mid vowel alternation has been leveled in favor of the high variant, and the mid vowel has been leveled out of the three way alternations (cf. Hooper 1976: 167). Given rule (10), Hooper claims, "the leveling is accounted for by the mere loss of the mid vowel case in each alternation. Subsequent to this loss, verbs such as contar are underlying /kwent-/, verbs such as pedir are underlying /pid-/, and verbs such as mentir are underlying

/m  $\{\overset{ye}{i}\}nt-/'$  (Hooper 1976:168). The rule given in (11) (Hooper's 44) applies to this latter class of verbs.

$$(11) \quad \left\{ \begin{array}{l} ye/we \\ i/u \end{array} \right\} \rightarrow \left\{ \begin{array}{l} ye/we \\ i/u \end{array} \right\} / [+stress]$$

Thus, she concludes, "the analysis involving rule (29) [here, rule (1)], based on Hudson's model, gives a uniform account of all three alternation types."

Hooper goes on to argue that "it is impossible to account for this leveling" if a diacritic analysis such as that of Harris (1969, 1974) is adopted. (In such an analysis, lexical representations contain a high vowel in cases where a high vowel appears as an alternant, and a mid vowel in the other case, along with a diacritic indicating that the vowel is subject to lowering, to diphthongization, or to both, and there are rules which lower high vowels to mid vowels and convert vowels under stress to the corresponding diphthongs if these vowels are marked with an appropriate diacritic.) She continues: "the diacritic representation...implies that through historical simplification, the diacritic will be lost, and the underlying form will replace all other allomorphs." While the developments



in some cases are in accord with this implication, this is not the case with respect to the former mid vowel/diphthong alternations (where there are now only diphthongs), since here the underlying form contained the mid vowel, and not the diphthong in Harris's analysis. Hooper argues further (p. 169) that no other diacritic analysis can account for this leveling, but I will not go into detail about this part of her argument.

Let us now attempt to reconstruct this argument in more explicit fashion. Given only the discussion in the next-to-last paragraph above, one might be tempted to reconstruct this argument as in (12).

- (12) If rule (10) (and the associated lexical representations) are correct, then a uniform account of the development of these alternation types can be given.  
A uniform account should be given (i.e., the development was uniform).  
Therefore rule (1) (etc.) are correct.

It should be noted that the minor premise in this reconstructed argument was never explicitly stated by Hooper. However, it is clearly needed to arrive at even an 'inductive' argument form (cf. the discussion in section 4.1.3 above), and Hooper apparently regarded it as being true.

If we examine the paragraph which follows this one, however, it becomes clear that the overall argument form which Hooper intends is the stronger Bayesian one. A revised reconstruction which takes this into account is given in (12').

- (12') Unless rule (10) (etc.) were correct, it would be unlikely that a uniform account of the development of these alternation types could be given.  
A uniform account should be given.  
Therefore rule (10) (etc.) are correct.

That is, no diacritic analysis can give a uniform account, and such an analysis is the only readily imaginable alternative. This, then, appears to be the form of Hooper's argument; and again the argument form appears to be a legitimate one; an assessment of the premises will be given in section 4.2.3.

4.2.2. Harris's reply. Before proceeding to Harris's reply, let me first sketch briefly his (latest) analysis, since he contrasts it with that of Hooper with respect to predictions about historical change. He maintains the basic diacritic approach outlined in the previous section: some lexical items (those subject to diphthongization) are marked with a diacritic [D], while others (and some which are also marked [D]) are marked with the diacritic [HM] (for 'high-mid'--these lexical items are subject to the high/mid vowel alternations). He has two separate rules which correspond to Hooper's rule (10); these are given in (13) and (14) (Harris's 4 and 10).

- (13) 
$$\begin{bmatrix} +\text{stress} \\ D \end{bmatrix} \rightarrow [-\text{syllabic}] \begin{bmatrix} -\text{back} \\ -\text{high} \end{bmatrix}$$



$$(14) \quad \begin{bmatrix} \text{V} \\ \text{-low} \end{bmatrix} \rightarrow \left\{ \begin{array}{l} [-\text{high}]/[\overline{\text{HM}}]C_1 \begin{bmatrix} +\text{syll} \\ +\text{high} \end{bmatrix} \\ [+high] \end{array} \right\} / [\text{X}[3 \text{ CONJ}]\text{Y}]_{\text{verb}} \quad \begin{array}{l} \text{(a)} \\ \text{(b)} \end{array}$$

That is, stressed e and o are converted to ye and we, respectively, in lexical items which are marked [D]; and in third-conjugation verbs which are marked [HM], a mid vowel appears when an i follows and a high vowel appears otherwise.

Let us now turn to Harris's counter-arguments. His basic approach is to first question Hooper's facts (cf. Stampe's criticism of Kiparsky in section 4.1.2). He agrees that the mid vowel has been leveled out of those alternations where a high vowel is one of the alternants (i.e., types (ii) and (iii) described in section 4.2.1). But, he goes on (p. 54), "in a number of dialects--overlapping, but not coextensive with those in which the high-mid alternation is lost--the diphthongization alternation has been partially lost. Here the loss is sporadic, affecting particular lexical items at random." The alternations have been leveled here in favor of the monophthong (contrary to Hooper's account), so that, e.g., "standard...qu[y<sup>é</sup>/e]r- 'want' has become qu[e]r-" The leveling of the mid vowel/diphthong alternation in favor of the diphthong mentioned by Hooper does in fact occur in some Chicano dialects, but it is somewhat more complicated than Hooper's account suggests: "this particular leveling has occurred only in first-conjugation verbs with the [w<sup>é</sup>] ~ [o] alternation... First conjugation verbs with the [y<sup>é</sup>] ~ [e] alternation, and verbs of the other conjugation with either alternation are not affected."

This requires that the account of the historical changes, assuming Hooper's analysis, be somewhat different. In particular, Harris claims, "the loss of the high-mid alternation consists of three seemingly unrelated changes: (a) loss of mid vowels in individual lexical disjunctions, (b) loss of the environment of rule 17 [here, rule (10)--DGC], and (c) loss of the second case of rule 19."<sup>12</sup> That is, contrary to Hooper's claims, her analysis does not give "a uniform account of" even just the high/mid part of the alternations. Furthermore, Harris argues, "nothing in this account predicts loss of the mid vowel in all forms, rather than the high vowel in some and the mid in others, or the high vowel in all forms."

Concerning the sporadic leveling of the diphthongization alternation mentioned above, Harris has little to say--he apparently feels that it is unproblematic for either his or Hooper's analysis (cf. pp. 54, 56). He does, however, note (p. 55) that "nothing in Hooper's account reflects the fact that diphthongization is lost in one particular morphological subclass, as described above," in the case of the leveling which affected only the o/we alternation in first-conjugation verbs.

Harris also argues that on his analysis, "loss of this alternation implies loss of all and only the machinery associated with just this alternation, namely rules 14<sup>13</sup> and 10a [here (14a)] and, obviously, the triggering feature [HM]. Nothing else changes. In particular, rule 10b [i.e., (14b)] remains in effect, guaranteeing that the only possible result of the loss of the alternation is precisely the survival of high vowels" (pp. 53-54). In the case of the sporadic leveling of diphthongization, "all that changes is that individual lexical items lose their exceptional diphthongization-marking property...This behavior in exceptional forms



is unremarkable" (p. 54). As for the levelings in favor of the diphthongs, Harris claims (p. 54) that "this means that, in addition to the general process of diphthongization (which remains unaltered), the grammar of these innovative dialects must contain a special statement referring to the absence of first-conjugation stems with the 'back' branch of diphthongization" (he refers the reader in a footnote to Harris (1974) for a formulation of such a "special statement").

Harris goes on to present some data not discussed by Hooper. Some speakers of the Chicano dialects just referred to, presumably due to influence from the mass media, schooling, and recent immigrants who speak more standard varieties of Spanish, have apparently reestablished the high/mid vowel alternation in a systematically characterizable way: "all diphthongizing third-conjugation verbs have high and mid stem-vowel variants; no non-diphthongizing verb does" (p. 56). That is, assuming that these dialects are more innovative than the other Chicano dialects which have leveled out the mid vowel variant completely, the only verbs which have reestablished the high/mid alternation are those third-conjugation verbs which in Harris's analysis were previously (and still are) marked with the diacritic [D]. Such facts, Harris maintains (p. 56), "are beyond the reach of Hooper's analysis, which treats all of the innovations in question as collections of accidents, rather than systematic changes."

Let us now summarize Harris's counter-arguments, and cast them in a more explicit reconstructed form. First of all, he claims that Hooper's analysis does not offer a uniform account of the historical changes with respect to the high-mid alternations, (i.e., that the major premises in (12) and (12') are false) and that (assuming that "all and only the machinery associated with just this alternation"--see above--is meant to be roughly equivalent to "uniform") his analysis does. This appears straightforward, and I will not attempt to explicate it further. His second point is that Hooper's analysis does not predict loss of the mid vowel in the high/mid alternations, while his does. This argument, as it stands, is of the form given in (15).<sup>14</sup>

- (15) Analysis  $A_1$  makes prediction P.  
Analysis  $A_2$  does not make prediction P.  
P is found to be true.  
Therefore,  $A_1$  is to be preferred to  $A_2$ .

It is of some interest to note that this argument, if strengthened somewhat, takes on a Bayesian appearance. Such a strengthened version is given in (15').

- (15') Unless Harris's analysis ( $A_1$ ) were correct, the facts P would be unlikely.  
P is true.  
Therefore,  $A_1$  is likely to be correct.

Harris apparently does not want to make this stronger claim, however, presumably preferring simply to establish the superiority of his analysis over Hooper's. It should be noted that in so doing Harris is making no claims as to the ultimate correctness of his analysis, but only that it is to be preferred to that of Hooper. His final argument appears to be that his analysis allows one to specify which verbs reintroduce the high/



mid alternation, while Hooper's does not. This argument form is sufficiently similar to that in (15) that I see no need to reconstruct it here (but note that "prediction" will have to be replaced by something like "generalization").<sup>15</sup>

4.3.2. Discussion. As noted above, Harris has called into question the accuracy of the data on which Hooper bases her argument. That is, the "development of these alternation types" referred to in (12) and (12') above does not correspond to their actual historical development. If the facts are indeed as Harris and his sources (which are the same, for the most part, as those of Hooper) describe them, then Hooper's argument is incoherent, since an implicit part of the major premise is not true. Even if the facts were exactly as Hooper has described them, however, there appears to be a problem for her, since Hudson's suppletion theory "proposes that the direction of leveling is always toward the form designated as the elsewhere case [i.e., the one without a conditioning environment--DGC] of the distribution rule..." (Hooper (1976:129)). This is not, on her account, what has happened in the diphthongization case--the levelin has been toward the diphthong, which is not the elsewhere case in any expansion of the "distribution rule" (10). Thus, either this case (and the similar case of the leveling out of the mid vowel in the three-way alternations) is a counterexample to this proposal and it must be modified, or the suppletion theory of lexical representations has been falsified. Note also that attempting to revise the suppletion analysis somewhat so as to make the diphthongs the elsewhere case would result in incorrect predictions concerning the sporadic leveling of the diphthongization alternations, where the diphthongs are leveled out and mid vowels remain. In any event, the fact that the diphthongization alternation levels out in different dialects in different directions indicates that the suppletion theory (or any other theory, undoubtedly) must give up its claims for predictive power concerning the direction of leveling.<sup>16</sup>

Let us turn now to Harris's arguments. First of all, does Hooper's analysis in fact make the loss of the high/mid alternation appear to consist of "three seemingly unrelated changes"? It seems to me that it does not; once the mid vowel in each "individual lexical disjunction" has been lost in the high/mid cases, the other changes follow automatically. That is, the fact that the environment of the second part of rule (10) is missing is an automatic consequence of the lack of lexical items containing both a mid and a high vowel in the braces of a suppletive lexical representation; there could never be an opportunity for this environment to be met, and so it is not needed. Similarly, the loss of the second part of Harris's rule 19 (see note 12) is the automatic consequence of there no longer being any mid vowels in the lexical representations of third-conjugation verbs. These changes do not appear to me to be at all unrelated. Moreover, Harris's analysis seems to be saying pretty much the same thing as Hooper's: once the "triggering feature [HM]" is lost, there is nothing for rule (5a) (his 10a) to apply to. Harris is not quite correct that his rule 14 has been lost, however. In actuality, it must have changed to something like (16).

$$(16) \text{ If } \left[ \begin{array}{c} 3 \text{ conj} \\ D \end{array} \right], \text{ then } \#X \left[ \begin{array}{c} +\text{syll} \\ +\text{high} \end{array} \right] C_o \#$$

That is, all diphthongizing third-conjugation verbs have a high vowel. Concomitantly, Harris's rule 13 (p. 46), given below in (17), must be modified to something like (18).



(17) If [D], then  $\begin{bmatrix} +\text{syll} \\ -\text{high} \\ -\text{low} \end{bmatrix}$

(18) If  $\begin{bmatrix} +1 \text{ conj} \\ D \end{bmatrix}$ , then  $\begin{bmatrix} +\text{syll} \\ -\text{high} \\ -\text{low} \end{bmatrix}$

That is, it is no longer true that all diphthongizing verbs have mid vowels--now, only first-conjugation verbs do. These changes again appear to be necessary consequences of the across-the-board loss of the diacritic [HM]. I can see little difference between the two analyses in this respect. It should be pointed out, however, that Hooper's analysis contains no analog of rule (17), and, it appears to me, it would not be at all easy to formulate a redundancy statement of the form which Hooper employs to express this fact. That is, it is apparently very difficult, if not impossible, to express in the notation used by Hooper the fact that whenever there is an alternation involving a diphthong in standard Spanish, there is also a mid vowel involved. (This is related only somewhat marginally to the arguments from historical change, but since rule (17) must be replaced by rule (18) for Harris, it cannot be completely ignored.) Thus, in sum, Harris's claim that Hooper's analysis does not provide a uniform account of the high/mid vowel alternations appears to be false.

The second issue to be discussed in this context is whether or not Hooper's analysis predicts the direction of leveling in the high/mid alternations. If we accept the principle that leveling favors the elsewhere variant (this principle has of course been shown above to be very difficult to maintain, however), then it seems clear that it does, since the high vowel is the elsewhere variant and it is the one that 'wins out' in the leveling. The fact that the principle which makes the prediction is probably incorrect should not be regarded too highly, by the way, since the corresponding principle for the diacritic analysis (that the underlying form shows up in cases of leveling) also appears to be incorrect in view of the data under discussion, since there are dialects which level out the diphthongization alternation in favor of the diphthong instead of the underlying monophthong. Thus, the second premise in (15) appears not to be true.

As for Harris's third argument, it is not at all clear to me that it is in fact the case that there is no way to specify which verbs reintroduce the high/mid alternation. In particular, just as Harris can state that precisely those third-conjugation verbs which are marked [D] reintroduce the alternation, it seems that Hooper can just as easily state that all third-conjugation verbs which contain a stem 'vowel' roughly of the form  $\begin{Bmatrix} \text{diphthong} \\ \text{high vowel} \end{Bmatrix}$  reintroduce this alternation. Again, the premise that Hooper's analysis cannot state this generalization appears to be false.

To sum up, when the data have been straightened out and the premises of the arguments are examined with these data in mind, the historical changes which have affected Spanish vowel alternations appear to shed little light concerning a choice between a Hooper-type suppletion analysis and a Harrisian diacritic analysis. Some general implications of these data in particular will be discussed below (see section 5.)



#### 4.3. Schuh-Leben

4.3.1. Schuh on 'rule inversion in Chadic'.<sup>17</sup> Schuh first presents data which indicate that in the Chadic language Kanakuru earlier stops "weakened to corresponding sonorants in phonologically specifiable environments," so that \*t, \*d, \*ɗ became r, \*k and \*g became ɣ, and \*p and \*b and \*ɓ became w. These weakenings resulted in many cases in synchronic alternations between stops and sonorants (e.g., yilik 'tongue' vs. yiliɣ-no 'my tongue'). But the synchronic rule, Schuh maintains, does not mirror the diachronic change; rather, "the rules producing the alternations...are 'hardening' rules and the sonorant variants are underlying" (p. 384).

In support of his contention about the rule having been inverted, Schuh offers three arguments: (a) in synchronic alternations, the sonorants currently alternate only with voiceless stops, regardless of their etymological source, and, furthermore, the sonorant/voiceless stop alternation has been extended even to etymological sonorants; (b) it would otherwise be impossible to distinguish stop-sonorant sequences which are subject to a rule of schwa-epenthesis from those which are not--only underlying stops (in Schuh's analysis) trigger epenthesis; and (c) plurals of certain nouns and verbs, which formerly contained various (etymological) stops, have been regularized so that they contain only p, ɗ and k (the singulars contain the corresponding sonorants); also, some singulars have alternate forms for the plural, one showing "hardening" and the other the sonorant found in the singular (cf. Schuh 1972:286-9).

Let us now attempt to reconstruct these arguments in more explicit fashion. Schuh's actual wording of argument (a) (p. 386) is repeated below in (19). The argument, in somewhat more explicit terms, can thus apparently be taken to be representable as in (20).

(19) ...regarding the contemporary Kanakuru consonant alternations as an inverse rule where sonorant → stop rather than the historical process stop → sonorant explains the regularization of the alternations giving only voiceless stops as alternates of sonorants. It also explains why etymological sonorants now alternate with stops.

(20) If the alternations are regarded as being due to an inverse rule, then the regularizations are explained. Therefore, the alternations are due to an inverse rule.

As stated, there appears to be something missing in this argument. For one thing, it seems clear that merely regarding the alternations as being due to an inverse rule cannot possibly play a part in an explanation of the regularizations (or anything else, for that matter)--these alternations must in fact be due to an inverse rule. It therefore seems desirable to amend the premise in (20) so as to delete the "regarded as being" part. I am reasonably confident that Schuh would have no objection to such an amendment; he was, after all, arguing that the rule inversion analysis is correct. The second thing which appears to be missing from this argument is a minor premise. Since it is easy to add a premise which would probably seem obvious to all linguists, and which would make the argument have roughly



the form of the inductive arguments discussed by Botha, it appears reasonable to add such a premise; the revised form of (20), including the revision in the major premise, is therefore given as (20').

- (20') If the alternations are due to an inverse rule, then the regularizations are explained.  
The regularizations should (must) be explained.  
Therefore, the alternations are due to an inverse rule.

Since the argument as given in (20') is inductive in form, it is subject to all the weaknesses of such argument types. In particular, it is quite possible that something other than the rule's being inverted could explain the regularizations. It is therefore possible that Schuh intended his argument to have a stronger Bayesian form, although it is difficult to tell since there are no connectives in Schuh's actual argument (I supplied the *if* in the reconstructions). To allow for this possibility, I give a Bayesian version of (20') in (21).

- (21) Unless the alternations were due to an inverse rule, it is unlikely that the regularizations could be explained.  
The regularizations should (must) be explained.  
Therefore, it is likely that the alternations are due to an inverse rule.

Let us turn now to Schuh's second argument. He presents (p. 386) the data given in (22).

- (22) a. a wupə-ro 'he sold (it) to her' [cf. wupe 'to sell']  
b. a gup-ro diyii 'he forged a hoe for her' [cf. guwi 'to forge']  
c. <sup>v</sup>ši kukə-mai 'he is learning it' [cf. kuke 'to learn']  
d. si duŋ-ŋai 'he is beating it' [cf. duyi 'to beat']  
[cf. also a duk-ro 'he beat (it) for her']

Schuh argues (pp. 386-7) that "if we were to take stops as underlying in all cases and derive the sonorants from them, there would be no way to distinguish the medial consonant in the verb root in [22a] from that in [22b] and the medial consonant in the verb root in [22c] from that in [22d] for the purposes of epenthetic ə insertion...Likewise, by not distinguishing k and ɣ underlyingly, we would have no way to predict which words have velars which assimilate to a following nasal, as in [22d]." What Schuh apparently means by this is that the underlying form of the verb stem for the above forms is the same as the surface form of the infinitive; after a rule which deletes final -i in verbs everywhere except pre-pausally has applied, the epenthesis rule mentioned earlier applies, breaking up stop-sonorant clusters,<sup>18</sup> and then a rule which assimilates velars to a following nasal and the inverted version of weakening (which applies in the complement of the former weakening environments) apply to produce the stops in the left column of (22). Sample derivations for (22a, b) are given in (23). This argument thus seems to be reconstructible in the modus tollens form given in (24).



(23)	/wupe-ro/	/guwi-ro/
i-deletion		guw -ro
epenthesis	wupə-ro <sup>18</sup>	
(assimilation)		
strengthening		gup -ro

- (24) If stops are underlying in all cases, there is no way to distinguish the required consonants with respect to epenthesis.  
These consonants should (must) be distinguished.  
Therefore, stops cannot be underlying in all cases.

The minor premise here is again only implicit in Schuh's actual words, but is so innocuous looking that it can reasonably be supplied here.

Let us now proceed to Schuh's third argument. As noted earlier, it has two parts: "first, the stops found in the plural [which have not undergone weakening] do not always reflect their etymologies....In fact, the alternations are always w/p, r/ɖ and ɣ/k" (pp. 388-9), with the exception of the verb 'to die', where r alternates with t. Secondly, "plural hardening ...involves an alternation which is both phonetically unmotivated and requires arbitrary marking of those lexical items which undergo it," which leads it to be replaced by "more regular processes which do the same semantic or syntactic work" (p. 389). In this case, the sonorants show up in the plural. Examples given by Schuh to support these claims include those in (25) (cf. pp. 387, 389--ngin is the most common and productive plural suffix).

- (25) 'hen' yaawe (sg.) yaapiyen/yaawingin (pl.) (cf. Tangale yabe)  
'gazelle' sere (sg.) sediyen/serengin (pl.) (cf. Dira kite)

It is not clear exactly what form these arguments should take here, since all Schuh does is mention the facts. It seems, however, that he intends something like (26), in which the arguments, of course, take a Bayesian form.

- (26) a. Unless the rule has been inverted, it would be unlikely that the (non-weakened) stops in the plurals would not reflect their etymologies and rather have a single stop for any sonorant in the singulars.  
They do not reflect their etymologies, etc.  
Therefore, it is likely that the rule has been inverted.
- b. Unless the rule has been inverted, it would be unlikely that the plural rules would be replaced by a more regular process in which the singular sonorant shows up in the plural.  
These rules have been so replaced.  
Therefore, it is likely that the rule has been inverted.

There is some evidence in Schuh's comment on Leben's reply that he did in fact intend a Bayesian argument at least for (26a), in that he argues (p. 289) that having an underlying stop and a rule of weakening in singulars "totally ducks the issue of why the stops in the plural have all been neutralized, and moreover why even historical sonorants now alternate with



stops..." That is, unless there had been an inversion, such changes would have been unlikely.

Schuh gives one final argument concerning historical change, this time in Hausa, another Chadic language. He first argues for a series of changes often referred to as 'Klingenheben's Law' (since these changes were first systematically described in Klingenheben 1928) on the basis of "synchronic alternates, dialect variants, and comparative evidence" (p. 391), whereby original velar stops became w, alveolar obstruents became ɾ, and labial stops, including etymological m, became w, all syllable-finally; there is again little question about whether this is an accurate description of the history of Hausa.

Schuh then proceeds to argue that this process has taken an inverted form from a synchronic perspective. One bit of evidence for inversion, Schuh argues, has to do with the formation of plurals. "The language is losing those plural forms where obstruents alternate with sonorants" (p. 393). The innovative plurals contain the sonorant which is found in the singular. Thus we find forms such as those in (27),<sup>19</sup> where the singular w is the result of Klingenheben's Law, and the original plural is formed by infixing -aa- and adding a suffixal -ee or -aa to the noun stem (t becomes c ([č]) and z becomes j ([j]) before front vowels by a general process of the language).

(27)	<u>Singular</u>	<u>Plural</u>
'buffalo'	ɓawnaa	ɓakaanee/ɓawnaayee
'heart'	zuwciyaa	zukaataa/zuwciyooyii
'Tuareg'	buwzuu	bugajee/buwzaayee

Moreover, Schuh continues (p. 394), there is further evidence "in the word gwauroo 'bachelor' which has the plural alternates gwauraayee and gwagwaaree. This second alternate has to be an analogical reformation resulting from the neutralization of \*P and \*K in syllable final position. The -u- in gwauroo comes from \*P, not \*g, as can be seen in the dialect variants gwabroo or gwamroo". Schuh again is not very specific about the actual form of his argument, presumably feeling that it should be clear from the form of his previous arguments. However, given the considerable lack of clarity concerning the precise form of his previous arguments, it is not at all clear whether this one should be reconstructed in an inductive, Bayesian, or modus tollens form, although it seems safe to assume that it is meant to be of one of these types. I give a Bayesian version in (28), mainly because, as will be seen in the next section, Leben appears to be construing it in this way.

- (28) Unless Klingenheben's Law has been inverted, it would be unlikely that the regularization of plurals and the analogical reformation of the plural of gwawroo would occur. They do occur.  
Therefore, it is likely that Klingenheben's Law has been inverted.

4.3.2. Leben's reply. Leben argues (p. 265) that "Schuh's evidence does not lead to the intended conclusion." He goes on (pp. 265-6) to claim



that "the positing of a synchronic stage with...inverse rules constitutes a middleman which it would be advantageous to eliminate in principle from the realm of possible phonological systems". In order to show that it is possible to eliminate inverse rules, of course, Leben must counter each of Schuh's arguments, and he proceeds to attempt to do so.

Concerning Schuh's first argument, Leben argues (p. 267) that "if we do not assume that sonorants became basic, it is still possible to explain the historical developments." Recall that what is to be explained is the fact that sonorants now alternate only with voiceless stops. Leben proposes an alternative explanation for this: "In the examples given by Schuh, the voiceless stops resulting from this regularization appeared in a typical devoicing environment, immediately preceding a voiceless stop.... If, in addition, etymological d, b, etc., ceased to surface phonetically as voiced stops, then future generations would be presented with no synchronic evidence for setting up underlying voiced stops in these words." Thus, the voiceless stops could become underlying for this reason in the case of etymological stops. As for the historical sonorants which now alternate with stops, Leben notes (p. 268) that "the only instances he cites of the extension of the alternation to historical sonorants occur in word final position, and Schuh himself notes (p. 386) that 'word final is a position of neutralization where stops and sonorants cannot contrast either phonetically or underlying [sic].'" The sonorants have been eliminated by this neutralization rule in favor of voiceless stops, which "will naturally be subject to the same alternations as any other instance" of voiceless stops. What Leben has done, then, is to argue that there is another possible explanation of the regularizations, i.e., that the if-clause in (20') can be replaced by something else (which would make this inductive argument unconvincing) or the major premise in (21) is false.

Similarly, Leben argues (p. 270) that it is possible to account for the varying susceptibility of stop-sonorant clusters to epenthesis without an inverse rule. In particular, he makes use of the same rules as those mentioned in connection with Schuh's argument, except that a rule of weakening, which mirrors the historical process, replaces Schuh's strengthening. This process, it should be noted, did not affect stops which were preceded by a short vowel and followed by e, or stops which were followed by e. These rules result in derivations such as those in (29).

(29)	/wupe-ro/	/gupi-ro/
<u>i</u> -deletion and epenthesis	wupə-ro	gup -ro
(assimilation)	-----	-----
weakening	-----	-----

The isolation form of /gupi/ weakens to guwi, but that of wupe (as in all other forms with stops between a short vowel and e) does not. This completes Leben's argument that the major premise in (24) is false.

Leben does not have much to say about Schuh's third argument, apparently feeling that inverted rules which apply only "in a small subset of nouns and in a small class of verbs that form plural stems" (p. 270) need not be eliminated from the class of possible rules. He does note, however, that he sees "no good reason for assuming that [the process at issue] did involve Weakening in singulars," and argues that "even if it were



shown that this morphological rule had become inverted, the case for Schuh's other inverse rules would not become any more plausible. For one thing, this morphological rule converts r into d, and thus it does not reinforce Schuh's earlier proposal of a rule to convert r into t." It thus appears that Leben is willing to concede that this could be a case of rule inversion, but argues that since the inversion has happened to a morphological rule, it is not of the same type as Schuh's earlier examples, which presumably involved phonological rules, and therefore is irrelevant to establishing whether or not it is possible for the latter type of rule to become inverted.

Concerning the Hausa example, Leben first argues (p. 274) that Schuh's "proposed solution seems unnecessary, since the regularized plurals are functioning to reduce allomorphy in the singular-plural paradigm." He goes on to point out that "there are perhaps over a dozen different ways of forming plurals in Hausa; a noun may take a number of different plurals, all with the same meaning. A given noun or adjective must be marked for which way or ways its plural is formed". Then he argues that "looking at the correspondence between the singular and the old plural and comparing this to the correspondence established with the regularized plural, it is hardly surprising that the regularized plurals should be gaining ground, to the detriment of the older forms." That is, he is apparently questioning the major premise in (28).

He goes on (p. 275) to "examine the question of whether rule inversion was even possible in the cases proposed by Schuh." His position is that it was not, since having a stage in the history of Hausa with inverse rules "entails eliminating an otherwise valid constraint on plural formation" (that stems which end in a consonant cluster have -aa- inserted between these consonants, while stems with a final glide-consonant sequence have -aa- inserted after the consonant in the formation of plurals) at this stage, while (p. 276) once this stage "began to be overcome by the regularization of the plural forms..., Hausa went back to the old restriction on -aa- Insertion..." "This scenario," says Leben (p. 277), "...is totally unacceptable." Moreover (p. 276), "the putative relaxation did not have any effect on the derivation of plurals that had pre-existing stems ending in a glide-consonant sequence" (that is, they continue to form their plurals by inserting the -aa- after the stem final consonant). This argument thus appears to be of a *modus tollens* type, as indicated in (30).

- (30) If Klingenberg's Law has been inverted, then the restriction on -aa- insertion was eliminated and then re-introduced, and the elimination of this constraint had no effect on pre-existing stems ending in glide-consonant. This did not happen (i.e., "is totally unacceptable"). Therefore, Klingenberg's Law has not been inverted.

Leben goes on to propose his own analysis of the development of Hausa plurals,<sup>20</sup> one which does not require an inverted rule, but rather makes use of "competing underlying forms" (p. 277). This proposal entails, for example, that "the existence of the covariants ɓakaaneɓ and ɓawnaayee in Hausa simply constitutes evidence for two competing underlying forms /ɓakn-/ and /ɓawn-/." Thus, there is an analysis other than Schuh's which is compatible with the regularizations observed to have occurred; i.e.,



Leben claims that the major premise of (28) is false. Finally, Leben suggests that his analysis, but not Schuh's, gives a possible explanation of why faŋkee 'trader' has only fataakee as a plural, and has not regularized. It has to do with the existence of "the derived form fatawcii 'trading' (derived by an unproductive process), where w comes from k in /fatk-/ by Klingenheben's Law. Therefore, restructuring of /fatk-/ as /faŋk-/, though it would succeed in reducing allomorphy in the singular-plural paradigm, would at the same time obscure the relationship of fatawcii to its root" (p. 278), while there is no corresponding form in the cases which are undergoing regularization. This appears to be a rather minor point in Leben's discussion, and so I will not attempt to make the argument form more explicit.

4.3.3. Discussion. Concerning the regularizations of the alternations in Kanakuru so that there is only a voiceless stop which, regardless of its etymological source, alternates with the sonorants which are the result of the historical weakening process (as well as etymological sonorants), there can be little question that Leben's account is superficially at least as plausible as that of Schuh. Schuh is apparently in agreement with this assessment, since he states in his comment on Leben's reply (p. 279) that "Leben has registered a number of valid criticisms of my analyses...", and does not explicitly comment on the issue in question. Thus, Leben has apparently succeeded in showing, at least to Schuh's satisfaction, that the if-clause in (20') is not the only possible means of providing an explanation for the regularizations, or, alternatively, that the major premise in (21) is false. It is possible, of course, that there could be data from Kanakuru which would be counterexamples to Leben's analysis-- there could be nonetymological voiceless stops which alternate with sonorants, and which are not in a devoicing environment, or there could be etymological sonorants which alternate with voiceless stops which are in other than word-final position. Schuh is apparently not aware of any evidence of the type just mentioned, since he does not bring it up in his comment, but this is clearly an empirical question. It should be noted in this regard that if there are in fact no data of this kind, Leben's account would appear to be supported, since the lack of stops occurring in other kinds of environments would appear a priori to be quite unlikely, unless perhaps some facts about the structure of Kanakuru preclude such data.

However, such data clearly do exist, even among the examples discussed by Leben, although he is correct in his statement that the examples cited by Schuh in his first argument contain no data of this type. Thus, we find guwi and a gup-ro diyii illustrating the w/p alternation (cf. Schuh, p. 385 and Leben, p. 268), where p is clearly neither in a devoicing environment nor in word-final position. Thus, since the alternating consonants come from etymological \*ḡ (cf. Schuh, p. 385), Leben's analysis cannot be maintained. He thus has shown neither that there is an alternative explanation for Schuh's facts nor, alternatively, that the major premise of (21) is false (i.e., he has not explained the disappearance of other stops from the stop-sonorant alternations).

The question of epenthesis is likewise in principle an empirical one, since Schuh's and Leben's analyses, though both generate the forms discussed by Schuh, make different predictions about the behavior of other possible



forms. Leben's analysis essentially claims that all verbs with stem-final -e or with a final consonant exhibit 'epenthesis,' while Schuh's predicts that verbs with etymological sonorants (which now alternate with stops due to the analogical leveling mentioned by Schuh) will not. That is, it is possible that, in addition to verbs with non-alternating stops (presumably (22a) is an example of this type) due to the inhibiting effect of the preceding short vowel and the following -e, there are verbs with etymological sonorants which alternate with stops. Leben's analysis predicts 'epenthesis', since the e would be between a stop and a sonorant at the point in the derivation at which this rule is applicable, but Schuh's analysis predicts none since what is an underlying stop for Leben is an underlying sonorant for Schuh in such cases. The two analyses also make different predictions concerning the behavior of verb stems in final -e, but with a long vowel preceding the pre -e consonant. These considerations show that there are potential differences between the two analyses even at the level of observational adequacy (one of them must generate incorrect surface forms), although both offer an account of the data at hand, with respect to the epenthesis facts. Choosing between the two analyses thus depends solely (or at least primarily) on synchronic data, and not on historical evidence in this case; both appear to be somewhat satisfactory accounts of the historical data presented by Schuh in his first two arguments.

Concerning the question of which account of epenthesis is in accord with the synchronic facts of Kanakuru, Newman (1974) gives some discussion which indicates that the facts are closer to being the way Schuh has described them, rather than as in Leben's analysis. First of all, it should be noted, the schwa-epenthesis rule is actually somewhat different than in either Schuh's or Leben's formulation--sequences of two consonants are subject to epenthesis if the first is voiceless or prenasalized, and the second may be any consonant, not just a sonorant as stated by Schuh. The sequence dr is also subject to epenthesis, as are all triconsonantal clusters (cf. p. 3). The exact statement of the epenthesis rule is not crucial to the present concern, however, and I will not pursue this matter further here. What seems most relevant in this respect is that Newman specifically states (p. 4) that "the invariant voiceless stops [i.e., those never in a weakening environment]...are...subject to [epenthetic schwa insertion], while the still unspecified archiphonemes are not." (Archiphonemes are used by Newman to represent the alternating consonants.) Thus, etymological sonorants (archiphonemes for Newman) which alternate with voiceless stops will not exhibit epenthesis even in words with final -e, contrary to Leben's analysis, but in accord with that of Schuh. Moreover, data presented elsewhere by Newman indicate that this is in fact the case, e.g., a<sup>h</sup>owe 'he tied it' (where the w is presumably an etymological w, since it is in an environment which prevented weakening), but a dop-təru<sup>21</sup> 'he (went and) tied it' (p. 9). Thus, Leben's account apparently cannot be maintained in this case, either.

As for Schuh's third argument (that concerning "plural hardening"), of course, Leben offers little objection to Schuh's analysis, and there is therefore not much to be said about it. The little that he does have to say (p. 270)--that he sees "little reason for assuming that [plural hardening] did involve Weakening in the singulars"--seems to be clearly



off the mark, since as Schuh (1974:280) points out, he did supply "cognate items from other languages" which suggest that there was indeed weakening in the singulars. Of course, Leben is not terribly concerned with this issue, since he apparently feels that inverted morphological rules are not of the type "which it would be advantageous to eliminate in principle from the realm of possible" (pp. 265-6) grammars of natural languages. And Schuh states (correctly, it seems to me) that Leben "is correct in noting that the plural rules...do not make rule inversion more plausible for the cases discussed earlier in the article..." (p. 290).

There is considerably more which can be said about the Hausa argument. First of all, it does indeed appear to me, at least, that "it is hardly surprising that the regularized plurals should be gaining ground," given the previous state of affairs which Leben describes. Thus, Schuh's major premise in (28) seems to be false. (This argument would not be any more successful if it had been reconstructed in one of the other forms, of course.)

On the other hand, Leben's contention that there could not be any inversion in the Hausa case seems to be on much less firm ground, and, of course, merely showing that the major premise in (28) is not true does not suffice to show that Schuh's analysis is incorrect. It is therefore of some interest to pursue this issue further, especially since, judging from Leben (1979), he feels that he has successfully shown that Schuh's analysis is an impossible one. Recall that Leben's claim concerning this issue is essentially that Schuh's analysis entails the loss of the condition of -aa- insertion at one stage in the history of Hausa and its reintroduction at a later stage. The question now is whether this sequence of events is really as unbelievable as Leben claims it is. On first glance, it does indeed seem that the reintroduction of a constraint of precisely the same form as one which has recently been lost from the language would be an extremely unlikely event. But if we look more closely at Schuh's account, it can be seen that this account appears to give an automatic explanation for the sequence of events in question.

Let us assume, then, following Schuh, that Klingenberg's Law has been inverted and that underlying forms have been appropriately restructured. Thus, the underlying form of the stem for ɓawnaa, for example, is /ɓawn-/. At the first stage of rule inversion, where the plural is still ɓakaanee, there must be a relaxation of the constraint on -aa- insertion, since it now must break up the glide-consonant sequence w-n, whereas before only consonant-consonant sequences could be broken up in this way. The derivation of ɓakaanee would thus include something like ɓawn- → ɓaw-aa-n- → ɓak-aa-n-, where the last change is the result of the inverse rules, at this stage. Leben's first question (p. 276) is why "the putative relaxation of the condition...did not have any effect on the derivation of plurals that had pre-existing stems ending in a glide-consonant sequence." That is, such forms continue to show -aa- after the stem-final glide-consonant sequence. The answer to this question seems to be simply that allowing -aa- to break up the glide-consonant sequences in such cases would result in an incorrect plural form; if the language learner is to have 'correct' plural forms, he must learn which glide-consonant sequences are broken up by -aa- and which add it after the final consonant, and those which fall into the latter category are of course precisely those which "had pre-existing stems ending in a glide-consonant sequence." Moreover, if a language learner were to 'make a mistake' in the direction of allowing older glide-consonant sequences to be broken up, the result would be an increase in allomorphy, since the glide would thereby be put in an environment which



triggers the inverse rules. Since, as Leben would apparently agree (p. 274), we expect changes of this type to reduce allomorphy, it seems not at all surprising that the older forms should retain the allomorphy-minimizing manner of -aa- insertion.

Can there be an equally plausible explanation for the reintroduction of the constraint on -aa- insertion? (This constraint would now be operative only in the speech of those who had completely regularized the plural system.) It seems to me that this reintroduction, given Schuh's analysis, is the straightforward consequence of the complete regularization of plural formation (note that the constraint cannot be operative in the speech of those who retain the older plurals for some of the singulars which were affected by Klingenheben's Law), which, as noted above, has a clear motivation--the reduction of allomorphy. That is, the result of the complete elimination of this type of allomorphy is that there happen to be no longer any cases where -aa- breaks up a glide-consonant sequence. Thus, the "generalization about -aa- insertion" was not, as Leben puts it (p. 277), "rediscovered," but simply reintroduced by a perfectly natural historical process, the reduction of allomorphy in the singular-plural paradigm. These considerations constitute essentially an argument to the effect that the minor premise in (30) is false.<sup>22</sup>

Since, as noted above, Leben has given an alternative analysis which does not make the regularizations in question appear unlikely,<sup>23</sup> it seems that in this case, as in the Spanish case, the data from historical change have little to say about the form of the synchronic grammar of the language in question before the changes. It is perhaps worth noting, however, that Leben and Schuh agree that the grammar of Hausa does not contain underlying forms which were exactly like those in the grammar before Klingenheben's Law took place; at least one positive conclusion about the form of the grammar of Hausa with respect to the underlying forms contained there, that not all of them are the same as before the changes occurred, can thus apparently be made.

#### 4.4. Skousen-Kiparsky.

4.4.1. Skousen on Finnish. Skousen's basic point is that many of the phonological rules which have been posited by generative phonologists to account for morphological alternations in Finnish are not part of a descriptively adequate grammar of Finnish; many of his arguments in support of this position invoke data from historical change, and it is these arguments which will be examined here. I will focus on Skousen 1972, since Kiparsky's reply is directed solely toward this work (he apparently did not have access to Skousen's 1972 dissertation, on which Skousen 1975 is based, when he wrote his reply), although reference will also be made to Skousen 1975 when it can be of help in clarifying the issues involved.

Before giving the arguments against the validity of the rules mentioned above, Skousen gives (p. 569) "some substantive evidence for a phonetically-plausible rule that speakers do capture," and I feel that it is worthwhile to take a brief look at this discussion before proceeding to his arguments against phonetically-plausible rules. The rule in question, a fairly old one found in Savo dialects, geminates a consonant "when it is preceded by a short, stressed syllable and followed by a long vowel or diphthong." Skousen's formalization of this rule is given in (31).

$$(31) \quad C \rightarrow C: / \check{V} \_\_\_ VV$$



Thus, for example, older téköö 'he does' has become tékköö in these dialects. Evidence that this rule (in this form) is still productive includes the fact that recent loan words undergo it, and that "more recent phonetic rules ... have set up surface exceptions to the rule of gemination," but "in every case, the rule of gemination eliminates these exceptions." For example, in some of the Savo dialects a word-final sequence Vns has become VVs, so that a word like våkens 'his people' (cf. standard Finnish våkensä) has become våkees in these dialects. "A surface exception to the rule of gemination has been created in the Savo dialects, but the rule of gemination has applied to give våkkees..." (p. 570). Thus, Skousen goes on, "there are phonetically-plausible regularities that speakers can capture." The form of this argument thus appears to be that illustrated by the reconstruction given in (32), which is more or less the classical *modus ponens* form.

- (32) If a (phonetically-plausible) rule applies to the output of a rule which enters the language after the rule in question, then it has been captured by speakers. Gemination applies to the output of a later rule. Therefore, gemination has been captured by speakers.

Skousen's first example of a "phonetically-plausible" rule which speakers do not capture concerns the well-known phenomenon of consonant gradation in Finnish, for which most generative phonologists have posited a rule which 'weakens' stops at the beginning of a closed syllable. The alternations to be accounted for (cf. Skousen 1972:571) are exemplified in (33).

(33)	p/v	tapa/tavan	'custom'
	t/d	pato/padon	'dam'
	k/φ	sika/sian	'pig'
	pp/p	piippu/piipun	'pipe'
	tt/t	lantti/lantin	'coin'
	kk/k	kirkko/kirkon	'church'

The above rule can be seen to account for these data: whenever there is an open syllable, the left member of the above pairs appears, while that on the right (the 'weakened' variant) shows up when there is a closed syllable. But this does not give an answer to the question which interests Skousen (p. 571), that of whether "speakers actually learn that gradation takes place in a closed syllable." He maintains that the answer is the negative, and that "speakers learn that stems take the weakened form when certain specific suffixes are added; for example, they memorize that the genitive suffix n and the inessive suffix ssä take the weak form of the stem without ever perceiving that both suffixes close the syllable." Skousen's argument in favor of his position has to do first of all with a change found in several of the western dialects of Finnish, whereby the inessive suffix ssä became sä, apparently underlyingly as well as on the surface. In these dialects, standard kädessä shows up as käresä (where r, rather than d, is the weakened form of t in these dialects). "This underlying inessive ending sä does not close the preceding syllable. Nevertheless, speakers of these dialects continue to use the weak form käre with this ending rather than the strong form käte. There is no tendency to change käresä to kätesä (p.571). "This suggests," Skousen continues (pp. 571-2), "that throughout Finnish, speakers are simply memorizing that the inessive suffix takes



the weak form of the stem--no matter how the inessive suffix may change." He goes on (p. 572) to give a similar example concerning the possessive suffixes, which, he claims, "are always added to the strong stem," regardless of how these suffixes affect the phonological environment of the relevant consonant in the stem, in many dialects, including the standard language. "There is absolutely no evidence that speakers ever change the system so that [suffixes which begin with a single consonant] take the weak stem." (I do not give the data with which this example is concerned, since it is so similar to the preceding example--see Skousen 1972:572 for the details). It is not clear from the discussion cited here what precise form Skousen intends his argument to take. However, he is more explicit in Skousen (1975:60) concerning a different example having to do with consonant gradation, and it is this more explicit presentation of the argument form which I will turn to now. I repeat this discussion here, replacing Skousen's "present passive form" (that being discussed in the passage under consideration) by 'inessive and possessive forms', which are the focus of the discussion at issue. Skousen states: "if the generative-phonological solution is correct, then the speaker would view the [inessive and possessive] forms as exceptional to the environment of gradation. And if the speaker must memorize this exceptional fact, we would expect some speakers...to change [these forms] so that [they] would conform to the environment of consonant gradation...However, there is no evidence for such changes..." The argument thus appears to be reconstructible as in (34).

- (34) If speakers regard gradation as taking place in closed syllables, then the inessive and possessive will cause changes to conform to this environment.  
Such changes do not occur.  
Therefore, speakers do not regard gradation as taking place in this environment.

This reconstruction, of course, has a *modus tollens* form. However, Skousen appears to want a considerably stronger conclusion, in addition to this one--that particular suffixes determine whether or not gradation takes place. In order to warrant this further conclusion, an additional premise to the effect that the "generative phonological solution" and Skousen's proposal are the only possible ones is presumably needed. If this is added--and Skousen seems to have operated under at least roughly such an assumption--then the additional conclusion does of course follow.

Let us now consider Skousen's argument against the second "phonetically-plausible rule" usually posited by generative phonologists, that raising word-final *e* to *i*. In some western dialects, word-final *k* has been completely lost (p. 573), so that "any word ending in *ek* would now be a surface exception to the rule of *e*-raising since the *k* would be missing. Yet in no case does the purported rule  $e \rightarrow i / \_\#$  apply to eliminate this surface exception." Another example of this type is "the allative suffix *lle*, which originally ended in a consonant that has now been deleted..." The final *e* is not raised here, either. If this argument is intended to be of roughly the same form as the previous one (and cf. also Skousen 1975:67-8), then it can be reconstructed as in (35).

- (35) If speakers have captured the rule of final *e*-raising, then surface exceptions to it will be eliminated.  
Such exceptions are not eliminated.  
Therefore, speakers have not captured this rule.



This argument thus again appears to be of the *modus tollens* type. Since Skousen does not propose an alternative analysis in this case, we need not be concerned with strengthening it as in the last argument.

The final argument to be considered concerns the standard generative rule which converts *t* to *s* before *i*. Skousen argues (p. 573) that "internally-created words such as *neiti* 'Miss' is [sic] never changed to *neisi*. Onomatopoetic words like *lotina* 'splashing' and *tippa* 'drop' have been created since the historical rule *t* → *s*/\_\_\_*i* applied, yet speakers never allow a rule like the historical rule to apply...Another example is the conditional ending *isi*, which originally came from *ńsi*, where *ń* represents a palatal consonant...The palatal *ń* was later changed to the high *i* vowel, but after the historical rule *t* → *s*/\_\_\_*i* had applied. Consequently, a verb like *pote* 'to be sick' has the conditional form *potisi*. Speakers never change this form to *posisi*." Since this argument is apparently intended to be of the same form as the previous ones (although there is no direct evidence in either of Skousen's works that this is indeed the case), I will not attempt a reconstruction of it.

It can thus be seen that all three of Skousen's arguments are apparently intended to have the same (*modus tollens*) form; this form is indicated schematically in (36).

- (36) If the standard analysis is correct, then certain changes would occur.  
No changes occur.  
Therefore, the standard analysis is incorrect.

4.4.2. Kiparsky's reply. Kiparsky begins by claiming (p. 92) that "a more thorough look at the problem indicates that...the rules which Skousen questions are very much in evidence as real synchronic processes of Finnish phonology." Before attempting to establish this claim, however, he first argues that "Skousen is surely right when he says that a phonological rule is real if 'surface violations' of it...tend to get eliminated. But the converse claim, also made by Skousen, that a rule is not real if surface violations of a rule do not tend to become eliminated, is too strong." For one thing, "the failure of a specific change to take place in a specific language at a specific period means nothing, since no one has been able to show conditions under which a change, however natural, must take place...the failure of surface violations of a rule to be eliminated cannot be used as proof that the rule is a linguists' figment" (pp. 92-93). Moreover, he continues (p. 93), "all 'surface violations' of a rule need not be exceptions to it, and if they are not, there is no reason why they should become eliminated." Thus, in the case of the *t* → *s*/\_\_\_*i* rule discussed above, although the examples Skousen adduces are indeed correct, "it is in those cases which are necessarily memorized, namely the morpheme-internal cases, that the *t* → *s* rule does not apply." Kiparsky's justification for the "necessarily" part of this claim is his argument earlier in his paper (cf. especially pp. 65-7) that "non-automatic neutralization processes apply only" when the environment is not "met already in the underlying representation of a simple morpheme." Since Skousen's examples are of this type, Kiparsky's proposal with respect to possible synchronic systems disallows the changes which Skousen maintains are necessary to establish the existence of a rule which has been "captured" by speakers; therefore, if Kiparsky's proposal is correct, "there is no reason to expect" Skousen's examples to undergo the *t* → *s* rule.



Kiparsky argues further, as alluded to at the beginning of this subsection, that there is external evidence for the rules which Skousen argues against. One rule which he discusses in connection with this concern is that of gradation. In the closely related language of Votic, claims Kiparsky (p. 94), "the inessive -ssa also went to -sa" (as in the dialects discussed by Skousen). Here the part of the gradation rule which affects nongeminates works as in the cases which Skousen described, but "the degemination rule has 'caught up' with the new form of the suffix and fails to apply, e.g., /nokka+sa/ nokkaza 'in the beak' (not \*nokaza, which would be the expected form)."

A similar sort of evidence, Kiparsky continues, is available concerning the t → s rule. In support of this contention, he argues (p. 94) that "there is one form class where the rule does seem to have been extended to new cases meeting its structural analysis which arose after the rule entered the language: the past tense form of vowel stem verbs." The stem vowel contracts (pp. 94-5), "under certain conditions, with the past tense suffix -i into i..." This i triggers the rule in some verbs, does so optionally in others, and does not allow it in still others. Kiparsky maintains that this contraction rule historically postdated the t → s rule, and that the latter has therefore applied, at least in some cases, to the output of an historically later rule. Since most historians of Finnish hold that the opposite sequence was the one which actually obtained (and, if so, it cannot be maintained that there is an historically later rule whose output the t → s rule affects). Kiparsky must argue against these contentions; he offers "five reasons why this view of the historical developments is the more likely one."

I will detail here only the fifth argument, which Kiparsky apparently feels is the most convincing, since he states (p. 99) that he believes that it alone would "suffice to establish the point." It concerns "the behavior of t before contracted i elsewhere than in the past tense." He states that "in all of these cases, not only is t the rule, but there are no traces of any kind, either in standard Finnish, or in dialects, or in the older literary documents, of the s which the standard theory claims must once have existed in them." He then asks "why this discrepancy between contracted i (in these cases) which never triggers t → s, and the contracted i of the past tense, which normally triggers s?" He continues that while

...the customary chronology offers no explanation; if instead we assume that t → s preceded contraction, the reason is clear. The difference between the past tense, where t → s was extended to new i's, and all other cases, where t → s was not extended, is that the change t → s before underlying /i/ happens to occur only in the past tense...Prior to contraction, therefore, the process t → s was applicable in the past tense, but in no forms in the other categories....After contraction, the situation was, from a surface point of view, that t went to s always before i from final e..., sometimes before i in the past tense ..., and never before i's in other categories (where all i's came from contraction)...Hence, a 'model' for the extension of t → s to new i's existed only in the past tense.

(Kiparsky 1973a:99-100)

That is, while verbs must be marked in the past tense as to whether or not they undergo t → s, and we thus expect elimination of these markings,



in the other cases there is a general (presumably morphological) characterization of the forms which do not undergo this process, and so we do not expect eliminations of 'exceptions' which do not really exist.

Kiparsky gives no discussion of the final e-raising rule.

4.4.3. Discussion. As noted in section 4.4.2, Kiparsky has no quarrel with Skousen's argument as reconstructed in (32). This suggests that the major premise in this argument should be considered as a potential 'universal' of historical change (see section 5 for further discussion).

But Kiparsky does of course take exception to Skousen's argument concerning consonant gradation. The issue in this case appears to boil down to whether or not the major premise in (34) is in fact true. First of all, there is the question of Kiparsky's proposed constraint on the applicability of "non-automatic neutralization processes" mentioned above. If this constraint is in fact legitimate, and if the only examples in which gradation has not been extended to conform to its putative "phonetically-pausible" environment do conform to the type of case contraindicated by Kiparsky's constraint, then it seems clear that we cannot expect the changes in many cases which Skousen's major premise in (34) predicts, and so this premise would be (at least partly) false. I know of no obvious counterexamples to Kiparsky's proposal, and my knowledge of Finnish is sufficiently limited that I have nothing further to say concerning either of the conjuncts of the if-clause in the above sentence.

Furthermore, as Kiparsky suggests, there does indeed seem to be some reason to question this premise even apart from this constraint, especially if there is no time frame referred to in this argument. However, since there seems to be an implicit such time frame in Skousen's argument, this consideration is open to question. That is, it is possible that an argument containing a premise to the effect that it is likely that certain changes would occur given a fair amount of time will be somewhat more legitimate than the one which Kiparsky argues against. Discussion of this issue, however, will be held off until section 5.

Moreover, given that Kiparsky objects even to the minor premise, further discussion of this issue seems to relegate the former question to a position of relatively minor importance at this point. The question thus currently concerns the truth or falsity of the minor premise. That is, has the gradation rule in fact been generalized, contrary to Skousen's claims? Kiparsky's argument that the answer to this question is in the affirmative, as noted above, concerns evidence from (p. 94) a "closely related language." There is thus some question as to exactly how closely related this language (Votic) really is to Finnish, but I can offer no further comment on this issue, since the reference which Kiparsky cites in this regard is written in Finnish. It should be noted in this respect, however, that if Votic is sufficiently closely related to Finnish in the relevant respects, then the minor premise in (34) does indeed appear to be false.

As for the rule converting t to s before i, Kiparsky does not comment on it with respect to his proposed constraint in his reply to Skousen. He does, however, use this rule as a supporting example when he argues for this constraint (pp. 61, 64), so he presumably would consider his objection discussed above as being relevant in this case as well. He also, of course, claims that this rule has in fact been extended to apply to the output of an historically prior rule, thus apparently contesting the minor premise



of Skousen's argument. This issue is, as Kiparsky points out, highly controversial, and in fact depends crucially on Kiparsky's account of the chronology of the rules involved. There is thus some question as to whether Kiparsky's contention in this respect is correct.

How reasonable is Kiparsky's account of the chronology, then? It seems to me that it is indeed correct that "the customary chronology offers no explanation" of the facts cited in the argument discussed above. Moreover, the other arguments which Kiparsky offers (which were not discussed here) also seem fairly convincing, for the most part. That is, the traditional account does not appear to have much to recommend it. However, it should be pointed out that Kiparsky's account requires that the historically later contraction rule be added to the grammar of Finnish in such a way that it precedes the  $t \rightarrow s$  rule. Since the latter is, according to Kiparsky, a "non-automatic neutralization rule," this account violates King's (1973) constraint against "rule insertion." Not only must there be such a rule insertion on this account, but all forms which would meet the structural description of  $t \rightarrow s$  as a result of the operation of the inserted rule must be marked as exceptions to the  $t \rightarrow s$  rule, since it is potentially fed by contraction in these cases (it is true, of course, as Kiparsky points out, that some of the exceptional cases are generally characterizable, but I am not sure how relevant this fact is in this context). A situation such as that just described would only be possible, as Kiparsky notes, if synchronic phonological theory is altered fairly substantially so that the more or less obvious synchronic description, assuming the possibility of extrinsic ordering of rules, of the state of affairs before the extension of the  $t \rightarrow s$  rule (i.e., ordering contraction after this rule) is not the most highly valued description in a case such as this. It should be noted in this regard that ruling out in principle counterfeeding orders (along with, e.g., Koutsoudas, Sanders and Noll 1974), an alternative which Kiparsky appears to give serious consideration, would entail that all of the "rule reorderings" out of counterfeeding order which Kiparsky and others have advocated cannot be the correct account of the changes involved. There are, however, as Kiparsky has also noted, other alternatives. In sum, Kiparsky's account seems to be less than totally convincing.

What is more, there seems to be a perfectly plausible way of accounting for the pattern of extension of the  $t/s$  alternations even if the  $t \rightarrow s$  rule is no longer alive synchronically, namely that the traditional concept of "analogy" is involved. That is, the  $t/s$  alternation is being extended only in the past tense because it is only here, as Kiparsky notes (see above), that there is "a 'model' for the extension of" this alternation. Formally, what is going on under this approach is sketched in (37), where  $V$  represents any vowel other than  $i$  and  $Q$  is the morphological category involved, with the obvious solution to this proportional equation being ' $x = s$ '.

$$(37) \quad tV : si]_Q :: tV : xi]_Q$$

Notice that the only occasion in which the left hand side of this formula will occur is when  $Q$  is past tense--there are no instances of  $[si]$  (only  $[ti]$ ) in the other morphological categories discussed by Kiparsky. Thus the facts which he cited in the argument discussed here seem amenable to an alternative account, if the chronology is as he argues it is. It should be noted that this solution does not require rule insertion, since



it does not require the existence of the  $\underline{t} \rightarrow \underline{s}$  rule and therefore of course does not require any particular ordering relationship between it and the contraction rule. It seems to me that the other data cited by Kiparsky would be amenable to a precisely analogous account.

I have no way of telling whether the Votic facts discussed in connection with the gradation rule are also amenable to this type of account, since Kiparsky presents very little data, and since it would be difficult, it not impossible, for me to find further data.

It should be pointed out that this type of account entails rejecting the premise that a rule is synchronically valid if surface violations of it tend to get eliminated. Further discussion of this question will be given in the next section.

5. Conclusion. It should be clear that all of the arguments examined in section 4, at least if they are interpreted perhaps somewhat charitably, can be considered as instances of elementary logically valid forms of inference--classical modus ponens and modus tollens (or, probably more realistically in many cases, the 'almost' variants of these mentioned in section 3), and the Bayesian form schematized in (2).

This situation, in my experience, is quite characteristic of arguments given by linguists (although there are, perhaps inevitably, some exceptions), and I suspect that it is generally true of arguments given in any science. This should not seem surprising, since linguists and other scientists are, at least indirectly (and sometimes directly), schooled in logical analysis. In fact, it seems to me that it is a good rule of thumb for a methodologist to follow that if it appears that an investigator is committing an elementary logical fallacy, then the methodological analysis itself is not unlikely to be faulty and that it therefore merits considerable scrutiny. I would suggest also in this respect that it is in general more likely that any lack of persuasive power felt concerning the arguments in question is probably due to a corresponding lack of belief in the truth of the premises involved in a logically valid argument form. That is not to say, of course, that investigators are never guilty of such logical fallacies (there are undoubtedly a number of quite genuine cases of this kind), but this need not compromise the value of this suggested rule of thumb as such. I would thus regard the putative discovery of an ever-increasing number of new types of fallacies (concerning mainly arguments in favor of extrinsic rule ordering--cf., for example, Koutsoudas 1972) as cases of somewhat misguided methodological analysis, and would maintain that what is actually involved here is disagreement concerning the truth of (implicit) premises in the arguments in question. This should not be taken as implying that such methodological studies have no value, for they have often provided good reasons for questioning the premises at issue (at least in my opinion), but terming such arguments 'fallacious' adds a rhetorical effect to the criticism that does not seem to me to be appropriate in such cases; investigators can differ with respect to beliefs about the truth of premises without one of them necessarily being found lacking in a necessary skill of the field--that of constructing logically valid arguments--which is not of course the case if he is in actuality guilty of a logical fallacy. Adopting such a rule of thumb, moreover, appears to be quite in keeping with the approach of many contemporary philosophers of science (cf., e.g., Suppe 1977), who place considerable weight on the actual practice of working



scientists. The work of the 'best' scientists is, not too surprisingly, held to be of the most importance by such investigators, and I would have to go along with them in this respect as well: we should be doubly skeptical when our methodological analysis entails that a classical argument (i.e., one which has met with considerable acceptance over a fairly long period of time) involves a logical fallacy.<sup>24</sup>

What is important as far as the evaluation of the arguments discussed above is concerned is thus the truth (or likelihood) of the premises involved. I would like to turn now to a brief consideration of this issue. This is an especially important concern, since at least some of these premises (or generalizations or revisions of them) are relevant not only to these arguments, but also potentially to other arguments which invoke diachronic data.

The major premise (5) of Kiparsky's argument in favor of the brace notation is, unfortunately (especially given the amount of attention it has received), not one of these. It appears to be quite specific to this particular argument, and I can see no obvious way of generalizing it. There is a lesson which can be gained from a consideration of this premise, however: the perceived cogency of an argument from historical change (or any other, for that matter) will vary with the degree of belief of each individual investigator concerning the truth of the premises involved. In this case, Kiparsky (and Chomsky and Halle) apparently found the relevant historical changes to indeed be quite unlikely unless the brace notation was psychologically real; Stampe, on the other hand, did not and was thus unconvinced by the argument--despite his acknowledgment of its "beauty". As pointed out above, evaluating the truth of this premise depends ultimately on (one's degree of belief in) the correctness of Kiparsky's (and Stampe's) synchronic analyses of Old English and Early Middle English. Moreover, in this case--and in many others, I would maintain--there is no clear way of establishing the required "correctness"--the problematic forms adduced by Stampe could always be given an (ad hoc) explanation, especially by a staunch syllable opponent. Whether or not a given investigator finds this argument (and other, perhaps all, arguments as well) convincing thus depends in the final analysis on essentially subjective factors.

As for the Hooper-Harris debate, the major premise (12') appears to be more readily generalizable, although even here what is probably the most obvious generalization--namely, that leveling not in the direction of the putative underlying form is problematic for a diacritic theory--is not terribly general. Here again the ultimate evaluation of this premise depends on subjective factors. The diacriticist could, for example, maintain that there is an implicit ceteris paribus clause attached to the prediction of direction of leveling, and that in the case(s) at issue there are other essentially irrelevant factors which are responsible for the 'failure' of the prediction. (In this particular case, in addition, the apparent similarity of the prediction made by the Hooperian analysis would likely mitigate the force of an objection based on this premise.) I can see no really objective way of determining whether or not these other factors are in fact irrelevant. The differences in directionality of leveling with respect to different dialects are suggestive, however, and I will return to this question below.

In the case of the Schuh-Leben controversy, what appears to be behind the major premise in (21) is something which is very similar to the principle



just mentioned: 'underlying' forms are favored in cases of analogical change. Similar sorts of questions can of course be asked about this principle. Leben's particular response concerning the directionality involved does have its problematic aspects, but this is not to say that there is no other analysis which does not require inverse rules which can make this directionality seem reasonable; the force of this argument depends on a subjective degree of belief in the likelihood of the possibility of such an alternative analysis. Similarly, an evaluation of the major premise in (24) (which does not seem amenable to generalization) depends on subjective factors, although, as pointed out above, Leben's particular synchronic analysis of the phenomenon in question is not without its problematic aspects. Here, too, however, nothing precludes a successful noninverted analysis. Since it appears that nothing new can be learned from a reconsideration of the remaining arguments given by Schuh, I will not discuss them here.

As noted in section 4.4.3, the major premise in (32) deserves serious consideration; I give a somewhat generalized form of this premise below in (38).

- (38) If a rule applies to forms which have appeared in the language, then it is psychologically real (i.e., "has been captured by speakers").

Unfortunately, it does not appear that it can be maintained. For one thing, how are we to tell if the if-clause in (38) is satisfied? The fact that a change in a form could be the result of the (productive) application of a synchronic rule is not sufficient to establish that the putative rule is indeed present in the language at the stage in question, as long as there is the possibility that there may be no rules involved in either the change or the synchronic alternations which preceded the change. It does not seem at all unlikely, moreover, that this possibility should be taken seriously, given that the direction of leveling does not appear to be predictable. That is, the different directions of leveling found in Spanish dialects (cf. section 4.2) could be taken to indicate, as Hooper suggests, that different speakers can have different rules, or that speakers have no rules at all for the phenomena at issue; I know of no clear way of distinguishing these two alternatives empirically (cf. note 16). To be more explicit about this new possibility, what I am suggesting (and I emphasize that at this stage it is only a suggestion) is that, except for productive processes and external sandhi alternations, speakers capture no psychologically real regularities (to use Skousen's terminology); i.e., other than in the cases just mentioned, speakers simply memorize the words of their language in roughly their classical phonemic representations, and they learn no rules (either morphological or phonological) to relate these words.<sup>25</sup> If a speaker does not know a word, then he quite literally uses analogy (cf. section 4.4.3) to come up with the required 'new' word.<sup>26</sup> Often the result of this use of analogy will be the word which already exists in the language, but sometimes speakers will not be 'successful' in their use of analogy, in that they come up with something that has heretofore not been accepted as a word of the language. In such cases, and if the new word meets with acceptance, there will have been an analogical change.

This possibility depends crucially, of course, on the nonpredictability of the direction of leveling, and I would like to briefly present some further evidence for such a nonpredictability.<sup>27</sup> The Spanish facts are



not isolated ones, and Skousen (1975) gives some discussion of some similar examples from the history of French. For example (p. 36), "the verb boire, according to the normal historical development, should have a future-conditional stem buvr-", and it did have for some time in the history of French, but it "has now been replaced by the stem boir-", so that the alternation in the stem for this verb has been leveled out in favor of the infinitive. However, we find (pp. 38-9) that "in Old French, infinitive forms like ardoir and saillir were replaced by ardre and saudre. The future forms of these verbs were historically ardra and saudra." Thus, here again the stem alternation has been leveled out, but this time in favor of the future. Skousen interprets these data as evidence that some speakers "learn that the future-conditional stem is the same as the infinitive" (p. 36), while others "have learned the reverse pattern--that the infinitive is based on the future-conditional stem" (p. 37), but unless we can predict which speakers are going to make which analysis, and/or which of these analyses will 'win out' in cases involving leveling, we are still not going to be able to predict the direction of leveling (cf. also note 16). Skousen gives at least two further examples of this type (cf. the discussion of the development of parler and raisonner, and of aimer and clamer (pp. 43-5)) as well.

Skousen's discussion of these levelings is of some interest, it seems to me, and I will digress here a bit to give some attention to this discussion. One of the main points of Skousen's monograph is (e.g., p. 41) that "...analogical changes [of the type discussed above] can be used as evidence for a psychologically real regularity between the infinitive and the future-conditional stem." Furthermore, he has (p. 41) "been using a restricted theory of analogy. In particular, [he has] assumed that analogical changes are not random, but occur in directions...Analogical changes occur when speakers remove exceptions to psychologically real regularities." He continues (p. 42) that "in this sense, analogical change is viewed not as the speaker's attempt to create a surface regularity, but rather as an attempt to eliminate surface exceptions to a surface regularity that has already been captured." Thus, Skousen appears to be proposing something quite contrary to what I have just suggested--any analogical change, for Skousen, suffices to establish the existence of a rule (= "psychologically real regularity"). This seems to be a stronger claim than Hooper's concerning similar facts (cf. note 16 and the related discussion), and also an incorrect one, at least if the concept "psychologically real regularity" is given a nontrivial interpretation.

Consider, for example, the development of the now quasi-productive -burger morpheme in English (cf. Jeffers and Lehisté 1979). There was of course originally a single word hamburger which consisted of two morphemes, hamburg and -er, but after this word had been (mis)analyzed as having the morphological composition ham+burger (presumably due to the presence of the word ham in the language), the new morpheme -burger has been extended so that there are now in English such words as cheeseburger and fishburger (cf. also Burger King, Burger Chef). Note that this development began with a single form, so unless we are willing to claim that a "psychologically real regularity" can be derived from a single example (and what could constitute an irregularity if this were the case?), it appears that Skousen's claim cannot be upheld. Another example of this type concerns the recently developed -(a)holic morpheme, which again appears to be the result of a misanalysis of a single form as alc+oholic (we now have words like workaholic,



gumaholic, etc.). (For discussion of a somewhat similar case concerning the development of the Latin infinitive, cf. Jeffers and Lehiste 1979.)

The upshot of all this is that (38), at least in its present form, cannot be maintained. But how about a somewhat weaker version? One could suggest, for example, that analogical changes which could be the result of the application of productive or external sandhi rules are evidence for the psychological reality of such rules. But even this has its problematic aspects. What is the difference between a productive rule and a nonproductive one? And how can we be sure that our proposed rule, and not another (more or less) empirically equivalent rule, is responsible for the change? Even the strongest tenable version of (38) must apparently be rather weak with respect to such factors, probably weaker than that just suggested, and will thus be of correspondingly little use in evaluating synchronic analyses.

One final alteration of this principle will be suggested here, one which in fact appears quite plausible to me, although I know of no conclusive reasons for maintaining it. (Here again, then, the success of arguments incorporating this principle as a premise apparently will ultimately depend on a subjective evaluation of the truth of this principle by each individual investigator.) The alteration involves the kind of changes involved, in particular whether or not they are across-the-board ones. The final version of this principle is given more explicitly in (39) below.

- (39) If a putative rule applies to every possible input for the rule which arises after the postulated entry of the rule into the language, then it is psychologically real.

In fact, it could be that Skousen had something of this nature in mind, rather than the (reconstructed) major premise in (32), since he claims (Skousen 1972:569) that "in every case, the rule of gemination eliminates [the relevant] exceptions" (emphasis added).

Let us turn now to an examination of the major premise in (36). This version undoubtedly should be generalized even further if it is to be considered as a universal principle of language change; a further generalization is given below in (36').

- (36') If a putative rule is psychologically real, then (relevant) changes will occur.

As noted above, Kiparsky rejects this principle, and it is sufficiently vague anyway (when will the changes occur?) that it would require revision in any event. It seems to me that a principle such as (36''), in which (36') has been made probabilistic and a time frame has been added, deserves serious consideration, although again it is not clear to me what sorts of evidence would be relevant to determining its truth or falsity.

- (36'') If a putative rule is psychologically real, then (relevant) changes are likely to occur within several centuries.

As noted above (section 4.4.3), Skousen may have implicitly maintained such a time frame. It is perhaps worth noting that even Kiparsky may subscribe to a principle such as this, since he did not stop at proclaiming that the major premise in (36) was false, but proceeded to argue that the



minor premise in this argument is also false. The answer to questions concerning the relevance of historical data to determining the structure of a synchronic grammar thus again appears to depend in large part on the subjective degree of belief of each individual researcher in the premises of the argument at issue.

The emphasis which I have given to the role played by subjective factors in explicating the relative convincingness of an argument will undoubtedly be disturbing to some, and I must admit that I am somewhat uncomfortable about it myself. But if it is believed that it is desirable that the philosophy of science reflect in large part the actual practice of scientists (as, again, is now widely held--cf. Suppe 1977 and the references cited there), then such a conclusion seems to me to be inescapable. Again, I do not mean to claim that investigators are never guilty of assigning truth values to premises in an intuitively undesirable manner, but I can see no nonarbitrary way of proscribing such 'undesirable' behavior. A fair amount of disagreement about the reality of received 'knowledge' in any given field thus appears to be inevitable, unless there is widespread agreement in the field concerning the premises which play a substantial role in the arguments advanced in these fields (i.e., unless there is a "paradigm" in a fairly strong sense of this term as used by Kuhn 1970). This is perhaps a somewhat pessimistic view to take of scientific knowledge, but if the cases considered here are at all representative of linguistic research, and if linguistics is a more or less representative science (as the non-linguistic arguments discussed in Churma 1979:Chapter II at least suggest), then such an approach seems to be inevitable. The truth (or likelihood) of at least the major premises in most scientific arguments (not just arguments from historical change) simply is not decided on in practice by 'objective' means, and for the most part is not in principle decidable in such ways. But the situation is not as chaotic under this view as the subjective nature of the evaluations might lead one to believe, and in fact the framework points to an intuitively reasonable procedure in cases of disagreement about the force of an argument. With the isolation of the crucial premises involved, proper attention can be given to research concerning these premises; an investigator who is convinced of the falsity of such a premise can seek out counterexamples, and an adherent of one can attempt to marshal evidence which makes the premise more likely. But we will never have certain knowledge in any empirical field, I would suggest.

It is worth noting in this regard that if the truth of the premises in the Bayesian arguments (and probably the others as well--cf. Churma 1979:Chapter II, Appendix) is objectively ascertainable, then the much discussed 'problem of induction' (cf., e.g., Salmon 1967) will have been solved. Given the lack of success in the solution of this problem in the past few hundred years, one could, it seems to me, reasonably look at a putative solution to this problem with a fair amount of reservation. The subjectivity required in my interpretation thus seems to me to be in some sense a virtue of such an approach, rather than a liability; while one might in principle desire certain knowledge, it seems that, as suggested above, in practice it is not possible, and that therefore an approach which claims to provide such knowledge is questionable solely on this basis. Thus, I would maintain that it is not possible to reduce inductive inference (in particular, Bayesian inference) to deductive inference, and that to hold that it is possible (as in establishing with certainty the probabilities involved in a Bayesian argument) involves a fundamental misconception. (For further discussion of this point, cf. Churma 1979 Ch. II.)



One final positive note deserves mention in this respect. This concerns the fact that, as Salmon (1967:122) puts it, in spite of the 'problem' that "the prior convictions of reasonable people can vary considerably," Bayes' Theorem entails that "as these individuals accumulate a shared body of observational evidence, the differences of opinion will tend to disappear and a consensus will emerge." That is, differences in prior probability assignments are more or less irrelevant, as long as the "shared body of observational evidence" is large enough; as long as enough evidence can be found, rational investigators will eventually be forced to have roughly the same degrees of belief concerning 'well-supported' hypotheses. Until such a body of evidence is accumulated, however, the main determinant of relative persuasiveness of a given argument will be the degree of belief of each individual investigator in the truth of the premises of that argument, and this degree of belief may, of course, vary from person to person. The premises of arguments which invoke data from historical change are no different from any others in this respect, and so the value of such arguments for the purpose of assessing the descriptive adequacy of proposed grammar fragments apparently must be tied correspondingly to the relevant subjective degrees of belief.

#### Footnotes

\*This paper is a revised version of parts of my dissertation (Churma 1979), mostly Chapter III. I would like to thank Fred Householder, Wayne Redenbarger, David Stampe, and Arnold Zwicky for helpful discussion of the issues involved.

<sup>1</sup>I extend here (naturally, I feel) Chomsky's concept of descriptive adequacy so that a partial grammar is descriptively adequate if it is (or would be) part of a complete descriptively adequate grammar. It is perhaps appropriate that I give here at least a working definition of what I consider a descriptively adequate grammar to be: following Chomsky and Halle (1965:99), such a grammar "...gives a correct account of the speaker's 'tacit knowledge'" of his language. The term 'tacit knowledge' here should prevent any misunderstanding which might arise if Chomsky's (1964:63) term "linguistic intuition" (Chomsky apparently considers the two expressions to be equivalent) is employed--the latter could create the impression that the issues at stake in phonological controversies may be settled by direct appeals to the "linguistic intuition" of native speakers about the constructs in question. On the contrary, most (if not all) contemporary phonologists appear to agree that intuitive judgments about the validity of these constructs cannot be made by speakers with any degree of reliability. The 'tacitness' of the knowledge involved is thus crucial.

<sup>2</sup>In my experience, it is almost always the truth of the premises involved which is at issue when disagreements as to the force of an argument arise--even if one of the adversaries claims that the other is guilty of a 'logical fallacy.' In addition, the implicitness of the premises in many arguments will often add to the difficulties. For further discussion of this question, see Churma 1979, Ch. III.

<sup>3</sup>For a similar discussion of the areas of child language acquisition and word games, see Churma 1979. For an essentially complete list of the types of data used as external evidence in the literature, see Zwicky 1975.



<sup>4</sup>Thus, the present study can be considered as an attempt to provide what Botha (1979b) calls "bridge principles" for relating synchrony and diachrony in phonology. It is worth pointing out here that this latter work (cf. also Botha 1979a) presents what appears to me to be a rather distorted interpretation of the views concerning external evidence expressed by Chomsky (1976). Contrary to Botha (p. 39), who interprets passages from Chomsky (pp. 5-6) as entailing that Chomsky feels that external evidence is not necessary "for the validation of mentalistic claims", it seems that what Chomsky actually intends here is that (p. 12), given the current state of linguistic research, internal evidence is likely to be more valuable, given the essential lack of relevant "bridge principles" in the case of external evidence. As Botha would undoubtedly be the first to admit, the relevance of external evidence to synchronic analysis is far from clear without such principles. And external evidence would be necessary, of course, in cases where internal evidence alone does not allow for a choice between two alternative accounts. Botha has also made some other rather disturbing misinterpretations of Chomsky's views on psychological reality and external evidence, in my opinion, but this is not the place to go into them.

<sup>5</sup>The only works of this type of which I am aware are the long (and for the most part misguided, in my opinion--cf. section 4.2.3 for details) discussion of an argument from historical change given by Kiparsky (1968) in Botha 1973 and the brief discussion of arguments from historical change in Sommerstein 1977. The latter discussion, though perhaps of some relevance to the concerns of this study, will not be examined here, since it appeals only to hypothetical examples; the cases examined here all come from actual arguments in the literature in favor of or opposed to a given analysis of natural language data.

<sup>6</sup>The "at least partially" qualification is intended to allow for 'supra-segmental' or 'prosodic' features of speech, i.e., ones which are not of the same character as the more 'ordinary' features traditionally made use of in linguistic analysis, such as those employed in Firthian prosodic analysis or Harrisian 'long components', and more recently in the work of Leben (1973) and Goldsmith (1976).

<sup>7</sup>It has often been argued that there exists a level of representation intermediate to the levels of lexical representation and systematic phonetic representation. Since these arguments do not make use of the type of evidence at issue here, as far as I know, they will not be considered here, nor will the issue be pursued further.

<sup>8</sup>The "somewhat" qualification is meant to reflect the fact that, if the probability that A is true =  $p$ , the probability that B is true given that A is true =  $q$ , and the probability that B is true =  $r$ , then  $r \leq p \cdot q$ . See Churma 1979, Appendix for a proof.

<sup>9</sup>Note that, if expressed in this way, the argument appears to say nothing at all about the psychological reality of braces, but only about whether or not they are "part of linguistic theory." This is a rather trivial problem (although Botha (1973) apparently does not agree, as will be seen in section 4.1.3), since it is clear from the last part of the



first paragraph quoted above that Kiparsky feels that the theory is making claims about psychological reality. In other words, a construct is part of linguistic theory if, and only if, it is psychologically real. When we combine this with the conclusion of the preceding argument (that the brace notation is supported as being part of linguistic theory), we of course have the conclusion that its psychological reality is supported.

<sup>10</sup>The requirement that E be at least fairly likely if H is true is not explicit in (2). In fact, as (2) is stated, this requirement can only be arrived at as the result of some sort of Gricean implicature (cf. Grice 1975). Such a requirement seems intuitively quite reasonable, and moreover appears to be supported by the more explicit version of (2) mentioned above. See Churma 1979, Ch. II for details.

<sup>11</sup>It is not clear that any data are of this type; see, for example, Kuhn 1970.

<sup>12</sup>"Rule 19" referred to by Harris is a lexical redundancy rule which expresses the fact that "only third-conjugation verbs have the mid/high and mid/high/diphthong alternations" (Hooper 1976:161). It is given formally in (i).

$$(i) \left\{ \begin{array}{l} \left[ \begin{array}{l} -\text{syll} \\ +\text{high} \end{array} \right] \quad \left[ \begin{array}{l} +\text{syll} \\ -\text{back} \\ -\text{high} \end{array} \right] \\ \left[ \begin{array}{l} +\text{syll} \\ -\text{high} \end{array} \right] \\ \left[ \begin{array}{l} +\text{syll} \\ +\text{high} \end{array} \right] \end{array} \right\} \text{Co]}_{\text{verb}} \text{ IS ALWAYS [+ 3rd conj.]}$$

It should also be pointed out that Harris's description of the change in the form of rule (1) appears to be different from that of Hooper, since she implies that rule (2) above is the only relevant rule which remains after the change, so that the entire second case, not just its environment, has been lost from rule (1).

<sup>13</sup>Rule 14 is a lexical redundancy statement which is roughly equivalent to part of rule (i) in note 12, and indicates (Harris 1978:47) that the diacritic "[HM]" appears only on the last nonlow vowel of a relatively small number of third-conjugation stems." It is given in (i).

$$(i) \text{ If [HM], then } \#X \left[ \begin{array}{l} \text{---} \\ +\text{syll} \\ -\text{low} \\ 3 \text{ conj} \end{array} \right] \text{C}_o \#.$$

<sup>14</sup>For a very similar formulation, cf. Harris (1978:56).

<sup>15</sup>There remains Harris's point that "nothing in Hooper's account reflects the fact that diphthongization is lost in one particular morphological subclass..." This appears to be quite true, but since there appears to



be nothing to prevent Hooper from adding to the grammar of the innovating dialects a "special statement" analogous to that of Harris to this effect, I will not pursue this point further.

<sup>16</sup>There is of course also the possibility that, as Hooper puts it (p. 167), "different speakers may arrive at different analyses of the same data...", so one could claim that a somewhat different rule--one in which the diphthong is in fact the elsewhere case in the relevant part of the rule--is present in the dialects which level in favor of the diphthongs. If this is true, however, it is impossible to disconfirm the theory with facts from historical change, given the essential lack of constraints on the form which rules may take, since it will always be possible to formulate a rule in which the favored variant is the elsewhere case. Thus, the theory would be making essentially no empirical claims, despite the invocation of 'external evidence,' as well as having no predictive power concerning the direction of leveling.

<sup>17</sup>For a fuller examination of the issue of whether "rule inversion in Chadic" has in fact occurred, see Churma (ms.)

<sup>18</sup>As Leben (1974:269) points out, this rule (or another one) must apparently also convert stem-final -e in verbs to ə between a stop and a sonorant consonant. Leben treats it as being part of the epenthesis rule, formulating it as in (i).

$$(i) \quad \left\{ \begin{array}{c} \phi \\ e \end{array} \right\} \rightarrow \text{ə} \text{ / } [-\text{son}] \text{ } \overset{C}{\text{___}} \text{ } [+ \text{son}]$$

(Schuh is not very explicit on any of the rules in this part of his article, and it is somewhat difficult to tell exactly what he intends.) It is not clear that these phenomena actually can be treated as a single rule, since there are cases where -e alternates with ψ (see below).

<sup>19</sup>Following Leben, I modify here the Hausa orthography used by Schuh to reflect the w output of Klingenberg's Law. The orthographic u is retained in direct quotations from Schuh, however, so it is of some importance to remember that his uu and au correspond to my (and Leben's) uw and aw.

<sup>20</sup>Leben (1977a, b) has recently given revised analyses of Hausa plurals in the framework of "upside-down" phonology (cf. Leben and Robinson 1977). Since this is not relevant to the present discussion, it will not be considered here. For some general criticisms of the upside-down framework, see Churma 1980b, Janda 1980.

<sup>21</sup>This is an example of the type alluded to in note 18, where the -e must be deleted rather than converted to schwa. It is not clear to me from Newman's discussion what the precise conditions on -e deletion are.

<sup>22</sup>For a perhaps equally plausible alternative account of these developments in the Hausa plural system within an inverted rule framework, cf. Churma (ms.).



<sup>23</sup> Since Leben's main concern seems to be to restrict as much as possible the range of permissible analyses of language data (by, in this case, eliminating the "middleman" of inverse rules "from the realm of possible phonological systems"--cf. Leben, pp. 265-6), it is not clear that Leben's own proposal does not violate the spirit of his enterprise; in what sense is linguistic theory restricted if inverse rules are disallowed, but "competing underlying forms" are not?

<sup>24</sup> Sadock (1976) is thus quite rightly bothered by the fact that his analysis of Halle's (1959) argument against the phoneme requires that it not correspond to a logically valid form of inference. For further discussion of Halle's argument and Sadock's critique, see Churma 1980a

<sup>25</sup> This suggestion shares some aspects of other currently proposed 'concrete' theories of grammar, in which words are listed in the lexicon in roughly their surface form (cf. Vennemann 1974b, Leben and Robinson 1977, Pollack 1977, Farrar 1978). My suggestion seems to differ from these proposals in (1) distinguishing nonproductive word-internal processes from all others, and (2) claiming that in such cases there are no rules at all, whether "via," "upside-down", or "redundancy."

<sup>26</sup> It has often been pointed out (cf., e.g., Jeffers 1974, Skousen 1975) that the term "analogy" has frequently been used carelessly in historical linguistics. However, if we are careful to make clear exactly what the analogy being appealed to is, it seems to me that this concept is not nearly as 'dangerous' as it has often been claimed to be (cf. King 1969: 139ff.). For further discussion along these lines (but in the context of historical linguistics), see Jeffers 1974

<sup>27</sup> It is true that there appear to be regularities (at least tendencies) concerning which forms are innovated (cf. Kurylowicz 1949, Watkins 1969, Hooper 1979). At least some of these tendencies (e.g., Kurylowicz's 'second law' that analogy proceeds from "formes de fondation" to "formes fondées") can perhaps be seen in terms of the framework suggested above to be the result of the (putative) fact that a speaker is more likely, based on considerations of relative frequency and perhaps other similar factors to know certain kinds of forms than he is to know other kinds (e.g., he would be more likely to know a "forme de fondation" than a "forme fondée").

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